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RECUEIL DES TRAVAUX DE l'Assemblée Générale tenue à Washington en Octobre 1927	PAPERS OF THE General Assembly held in Washington in October 1927

FASCICULE 1

Mémoires scientifiques présentés à la séance publique. (TEXTES ORIGINAUX)	Scientific papers presented in the public session. (ORIGINAL TEXTS)

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TEXTES ORIGINAUX (1)

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presented in the public session of October 13th 1927

ORIGINAL TEXTS (1)

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The existence of more than one ionized layer in the upper atmosphere,

by Profr. E. V. APPLETON, F. R. S.
(Great Britain)

During the past year and a half systematic observations have been made at the Peterborough Research Station on the characteristics of wireless waves deviated by the upper atmosphere. As part of such routine measurements many determinations of the equivalent height of the Kennelly-Heaviside layer have been made, utilizing special transmissions from the National Physical Laboratory and from the stations of the British Broadcasting Corporation. The early summer observations of 1926 showed that the night-line height of this deviating layer (called, for convenience the E layer), for wave-lengths of 400 meters, was usually 90 kilometers to 130 kilometers. During the period 6 October 1926 to May 1927, however, heigths of an entlrely different order of magnitude, namely 250 kilometers to 350 kilometers, have been frequently measured during the three hours before dawn.

On such occasions, after the high values have been recorded for two or three hours, a discontinuity in the series of values occurs 20 to 40 minutes before sunrise, and heights of the normal value are again recorded.

The experimental evidence leaves little doubt that on such occasions, in the period before dawn, the ionization in the Kennelly-Heaviside layer (Layer E) has been sufficiently reduced by recombination to permit of its penetration by waves of this frequency. « Reflection », however, takes place at an upper layer (F layer) which is richer in ionization. With the advent of sunrise at a height of 100 kilometers or so, the Kennelly-Heaviside layer (layer E) is formed again and deviation by this layer is suddenly established. The normal fall of its under- boundary proceeds as the more direct solar influence increases the region ionised. As the day further proceeds the experimental results show that another region of ionization (D layer) is formed below the Kennelly-Heaviside layer, which, while causing attenuation of the waves, does not materially affect the height at which they are deviated. Occasionally « reflected » waves are detected from this layer. Its main function is, however, to absorb the waves which are only slightly bent in passing through it, and which are finally deviated by Layer E. It is suggested that layer D is the same as the ozone discovered by Lindemann and Dobson.

* * *

The influence of the earth's magnetic field on wireless transmission (Summary),

by Profr. E. V. APPLETON, F. R. S.
(Great Britain)

The magneto-ionic theory of the propagation of wireless waves in an ionized medium proposed by the writer (*) and by Nichols and Schelleng (**) is extended to the general case of propagation in any direction with respect to the magnetic field. The symbols used have the same significance as those used by H. A. Lorentz (***) except that for free electrons it is shown that we must write $g = \frac{m}{\tau}$ (and not $g = \frac{2m}{\tau}$ as used by Lorentz for bound elctrons). When absorption is neglected the formula for the refractive index reduces to

$$\mu^2 = 1 + \frac{2}{2\alpha - \frac{\gamma_T^2}{1+\alpha} \pm \sqrt{\frac{\gamma_T^2}{(1+\alpha)^2} + 4\gamma_L^2}}$$

where γ_T and γ_L refer to the terms involving the tangential and longitudinal components of the magnetic field respectively. There are thus two waves propagated, in general.

The above formula shows that μ^2 becomes infinite when

$$\alpha^2 = \gamma_L^2 + \left(\frac{\alpha}{1+\alpha}\right)\gamma_T^2$$

so that only when ($\alpha \gg 1$) do we get

$$\alpha^2 = \gamma_L^2 + \gamma_T^2 = \gamma^2$$

and quasi-resonance with the total field. This means that only when the ionization is small

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(*) *Proc. Phys. Soc.*, 37, February 1925.
(**) *Bell Technical Journ.* 4, 215, April 1925.
(***) « *Theory of Electrons* », pp. 139 et seq.

should critical effects occur for one of the waves at about 200 meters.

The formula further shows that μ^2 becomes zero, indicating possibility of reflection even at vertical incidence on the layer, when

$$1 + \alpha = 0,$$

or

$$1 + \alpha = \pm \gamma$$

relations which involve only the total magnetic field.

The marked absorption in the D layer will act differentially on the two waves which are, in general, transmitted, so that for wave-lengths in the broadcasting band in Europe we may expect only the non-critical ray to reach the ground in appreciable intensity. In this way the results of Meissner, who found no anomalies at 212 meters, may be explained.

* * *

On the influence of Solar activity on Radio Transmission (1),

L. W. AUSTIN and Miss I. J. WYMORE
(Laboratory for Special Radio Transmission Research,
Bureau of Standards, Washington D. C. (U. S. A.)

At the meeting of the American Section of the U. R. S. I. in April, 1927, some curves were shown (2) indicating a relationship between very long wave transatlantic radio reception and solar activity as measured by sunspot numbers and by the values of the solar constant. These curves represented annual averages from 1915 to 1926, and monthly averages from 1922 to 1926. These indicated without much question that there exists a direct correlation between solar activity and daylight signals, when averaged over long periods. Curves were also shown illustrating the correspondence of 27-day periodic averages of sunspots and reception.

G. W. Pickard (3) had previously published a paper using his own observations in the broadcasting band, as well as the observations of others, on very long as well as ultra short wavelengths, in which he produced striking evidence of the dependence of radio phenomena on solar activity. Mr. Pickard's own observations indicated quite positively an inverse relationship between the strength of night signals in the broadcasting band and sunspot numbers, while the ultra short wave night signal material treated by the same methods, indicated a direct correlation. This last conclusion is, however, seemingly not in agreement with a number of observations made by others of weakened ultra short wave night signals at times of great solar and magnetic disturbances.

During the past summer the examination of the long wave observational material at the Bureau of Standards, in connection with sun spots, has been continued. Especial attention is now being given to the correlation of the daily observations, or the averages of a few days at most, rather than to long time averages. It is found in general that daily comparisons of signals and sun spots give too complex curves to be of use. But when they are averaged in five-day periods good results begin to appear. It is also found in the examination of the material that the stations at moderate distances (less than 1,000 km.) when treated in this way give on the whole better correlation with sun spots than more distant stations.

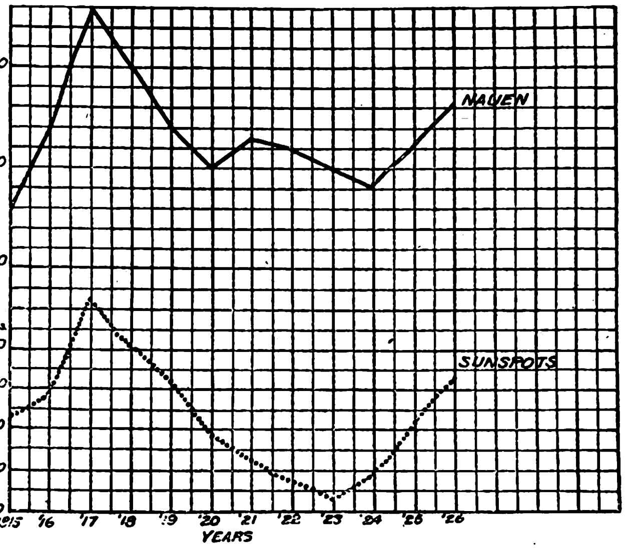

Fig. 1. — Annual Average Signal Intensity of Nauen (AGS), 10 A.M. and Sunspot Numbers.

Since the signal and sunspot curves do not follow each other entirely in detail, it seems probable that there are other factors besides solar activity which contribute to the signal variations. One of these we believe to be temperature which has already been shown (1) to have a strong influence especially in winter on the reception in Washington of the transatlantic stations at New Brunswick and Tuckerton, New Jersey.

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(1) Read at the International meeting of the Union of Scientific Radio Telegraphy in Washington, October 14, 1927.

(2) Proc. I. R. E., vol. XV, p. 825, 1927.

(3) Proc. I. R. E., vol. XV, p. 83, 1927. See also vol. XV, p. 749, 1927.

There also seems to be a tendency for the sunspot and signal curves to shift relatively back and forth in phase as though at times there might be a lag in the appearance of the sun spots in respect to the fundamental solar disturbance producing them, while on the other hand, there often seems to be a delay in the formation of the conditions in the earth's atmosphere which produce the changes in signal strength. At any rate there is no doubt that at times the increase in signal precedes the increase in sun spots, while at other times the reverse seems to be true.

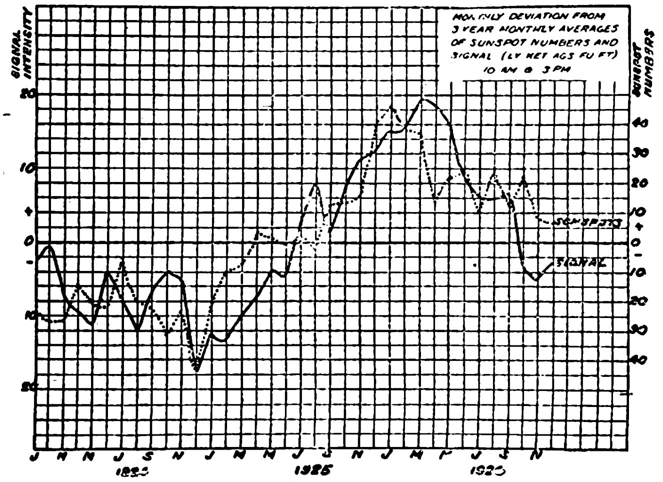

FIG. 2.

Notwithstanding these phase shifts, the frequent similarity of the sunspot and signal curves renders it almost certain that we are dealing with a genuine relationship. On the whole there seems to be about the same degree of correlation between the signals and sun spots as between signals of similar wavelengths received at a given receiving station from two separate transmitting stations. In the following, Figs. 1, 2, and 3 deal with long time comparisons in which the connections between solar activity and radio transmission is believed to be fairly well established, while Figs. 4, 5, 6, and 7 deal with short period (5-day) averages in which the transmission curves are smoothed by the smoothing formula $\frac{a+2b+c}{4}$. This last, without changing the positions of the main maxima and minima, renders the transmission curves more easily comparable with those of the sun spots.

Fig. 1 shows the annual averages of Nauen ($\lambda = 13{,}000$ m.) as received at Washington at 10 A. M. from 1915 to 1926, and the corresponding annual averages of the Wolfer sunspot numbers covering the same period. The signals from 1915 to 1921 were taken by the shunted telephone method and are therefore less accurate than those taken later, but we believe that there can be no doubt that the general course of the curve is correct.

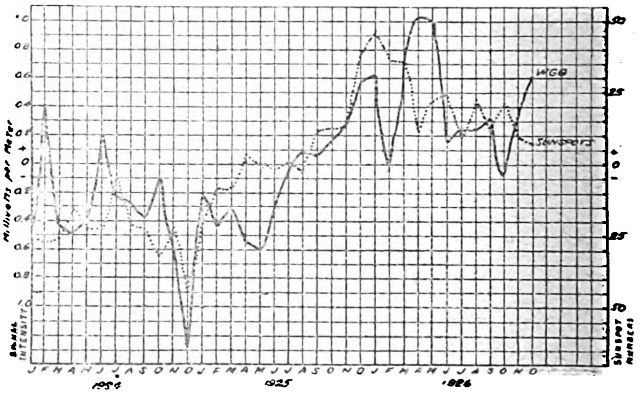

FIG. 3. — Monthly Deviation from 3-Year Monthly Averages of Tuckerton (WGG), 10 A.M. and 3 P.M. (Corrected for Variations in Antenna Current) and Sunspot Numbers, 1924-1926.

Fig. 2 (1) shows the deviations of the individual monthly averages from the three-year monthly averages of five distant stations as compared with the sunspot numbers. It is seen that, while the two curves do not follow each other in detail, in general they rise together during the three years of increasing solar activity. The portion of the figure dealing with the rapid increase of sun spots in the autumn of 1925 is of especial interest.

Fig. 3 shows similar curves for the Radio Corporation of America transatlantic station at

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(1) PROC. I. R. E., vol. XIV, p. 781, 1926.

(1) Figs. 1 and 2 are repeated from PROC. I. R. E., vol. XV, p. 825, 1927.

Tuckerton, N. J. (WGG), only 250 km. from Washington.

As has been said, the degree of correlation between sun spots and signals is comparable with that between two signals received at one point from different transmitting stations. For comparison with the following curves, therefore, Fig. 4 is shown giving the relationship of the five-day averages of Tuckerton, WGG, and Bordeaux LY as received at 10 A. M. during a portion of 1925 at the Bureau of Standards.

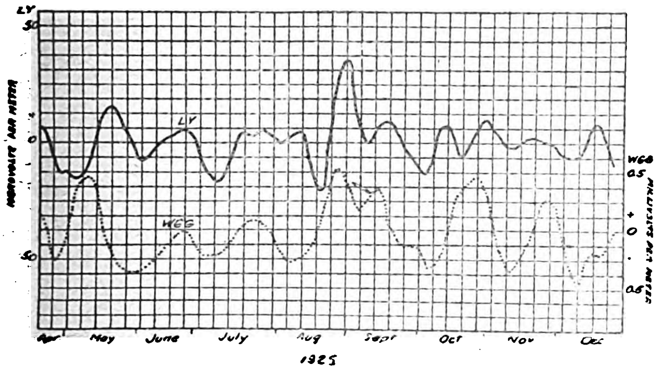

Fig. 4. — Deviation of 5-Day Averages from Monthly Averages of Tuckerton (WGG), 10 A.M. and 3 P.M. and Lafayette (LY), 10 A.M. (1925).

Fig. 5 shows the deviations of the five-day averages of Tuckerton as compared with the five-day averages of the sun spots in 1924. The year 1924 was very near the sunspot minimum.

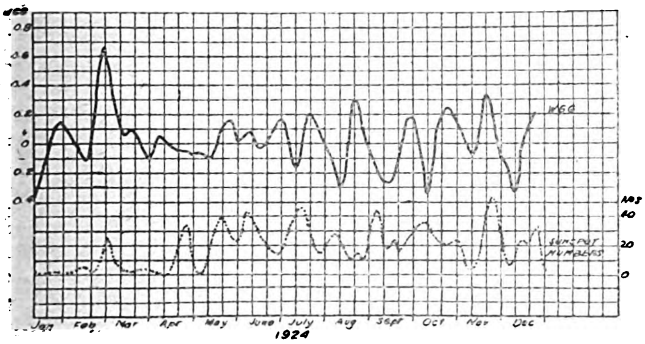

Fig. 5. — Deviation of 5-Day Averages from Monthly Averages of Tuckerton (WGG), 10 A.M. and 3 P.M. and 5-Day Averages of Sunspot Numbers, 1924.

Fig. 6 shows the relationship between the sun spots and the same station between March, 1926 and March, 1927. Here the curves are drawn on a smaller scale than in 1924 on account of the much greater amplitude of the variations both of the sunspot numbers and of the signal intensity. It is at once seen that the degree of correlation of the sunspot numbers and signal seems much better in 1926 and 1927 than during the period of low solar activity in 1924.

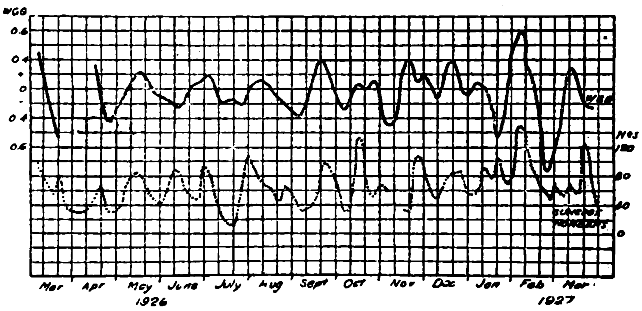

Fig. 6. — Deviation of 5-Day Averages from Monthly Averages of Tuckerton (WGG) and 5-Day Averages of Sunspot Numbers, 1926-1927.

Fig. 7 shows similar curves for the R. C. A. station WSS at Rocky Point, L. I., at a distance of 435 km. from Washington in 1926-27. In addition to the signal and sunspot curves, a curve of the five-day averages of the range of the horizontal intensity of the earth's field as observed at Cheltenham, Maryland, is included.

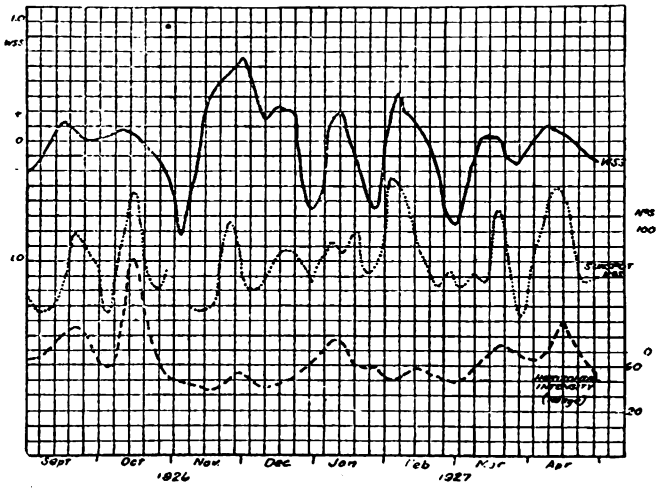

Fig. 7. — Deviation of 5-Day Averages from Monthly Averages of Rocky Point (WSS), 10 A.M. and 3 P.M. and 5-Day Averages of Sunspot Numbers and Horizontal Intensity (Cheltenham), 1926-1927.

The resemblances between these curves are really striking, though we find here also the relative shifts in phase already mentioned between the sunspot numbers and the signal intensity. In some cases the signals evidently lead, while in other cases they lag.

In examining the curves of the five-day

averages, it is evident that while there is little question of a connection between solar activity and radio signal intensity, yet there is a lack of any direct proportionality between the sunspot numbers and signal strength. For it is not by any means always the largest increase in sun spots which corresponds to the largest increase in radio transmission.

Our present knowledge concerning the relation between solar activity and the strength of radio transmission may be briefly summarized as follows :

It seems reasonably certain that long wave daylight signal strength has increased in recent years with the increasing solar activity, when averaged in periods of a month or more, and there is fair evidence that the annual averages of signal intensity have roughly followed the sunspot curve since 1915.

It is also reasonably certain that night transmission in the broadcasting range when averaged in periods of a few days is reduced at times of high sunspot numbers.

The information regarding the effects of the sun on long wave daylight transmission and ultra short wave night transmission averaged in periods of a few days, is somewhat discordant differing at different times and on different stations. These discrepancies may be due to the relative phase shifts in the transmission and sunspot curves already mentioned.

Very few observations have been published on long wave night transmission, on broadcasting day transmission, nor, so far as is known, have any been made on broadcasting night transmission extending over more than one year.

There is in general a rough resemblance observed in the reception curves of different long wave stations lying in different directions from Washington. This would seem to imply either that there are simultaneous changes in the electrical conditions in the upper atmosphere over very large areas or that the conditions in the immediate neighborhood of the receiving station form the chief controlling factor in signal strength. Occasionally, however, marked differences in reception from stations in different directions have been noted, a phenomenon which is much more common in the case of the broadcasting and ultra short waves at night.

While the sunspot numbers give a rough indication of the conditions governing transmission, it seems probable that they are only an imperfect index of the particular form of solar activity which apparently is the chief agent in controlling radio phenomena.

* * *

Relations entre les parasites atmosphériques et les phénomènes météorologiques,

par ROBERT BUREAU.

L'étude directe des atmosphériques m'a conduit à répartir la plupart d'entre eux en trois catégories différenciées par les caractères de leur variation diurne.

On distingue ainsi : des *atmosphériques nocturnes* qui naissent vers le coucher du soleil et meurent à son lever — des *atmosphériques irréguliers* que l'on voit apparaître ou disparaître à toute heure du jour ou de la nuit — des *atmosphériques d'après-midi* qui surgissent brutalement vers midi et qui faiblissent vers 21 heures (fig. 1).

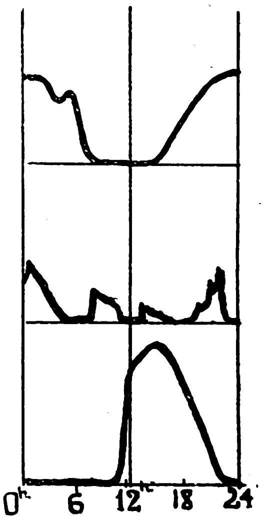

FIG. 1.
Représentation schématique des variations diurnes des trois catégories d'atmosphériques.

Par ailleurs j'ai été amené à rattacher les atmosphériques à trois types de situations météorologiques qui correspondent aux trois genres de variation diurne ci-dessus indiqués. Nous verrons ci-après que les atmosphériques nocturnes sont particulièrement nets dans les *situations anticycloniques*, que les atmosphériques irréguliers se rattachent aux courants de *perturbations du front polaire*, et que les atmosphériques d'après-midi naissent dans certaines *perturbations orageuses* indépendantes du front polaire.

Comme l'étude des caractères météorologiques des atmosphériques nous conduit ainsi

à la même classification que l'étude de leur variation diurne, il y a tout lieu de supposer que les caractères de la variation diurne ne sont qu'une conséquence des causes météorologiques différentes qui provoquent l'activité plus ou moins grande des atmosphériques. Nous verrons qu'il en est bien ainsi; que, dans les trois cas, les atmosphériques sont d'autant plus violents que l'atmosphère est soumise à une agitation plus forte suivant la verticale, autrement dit que son instabilité thermique suivant la verticale est plus grande. Cette instabilité thermique peut elle-même avoir diverses causes et les principales sont :

a) La *turbulence* sous les surfaces d'inversion de température (et en particulier sous les surfaces d'inversion anticycloniques);

b) *L'instabilité verticale* due à l'arrivée de masses d'air polaire en des régions relativement chaudes ou due au passage de certaines surfaces de discontinuité;

c) Les violents *mouvements verticaux de convection* dus à l'échauffement solaire violent des couches basses par l'intermédiaire du sol.

1. — Les facteurs météorologiques.

Il importe de noter que si l'on a pu établir d'une manière précise les relations étroites qui rattachent les parasites atmosphériques aux phénomènes de l'atmosphère, c'est que les progrès récents de la météorologie nous ont fait connaître ces phénomènes et leur mécanisme avec une grande précision. Nous savons en particulier qu'il ne faut plus considérer la perturbation météorologique classique, c'est-à-dire la dépression barométrique, comme un individu météorologique formant un tout homogène, mais que, tout au contraire les dépressions barométriques sont des régions où se heurtent des masses d'air d'origine, de nature, et de caractères physiques différents; que l'identification de ces masses d'air est devenue l'un des buts principaux de l'analyse météorologique; et que leurs surfaces de séparation provoquent au sol et en altitude des surfaces de discontinuité dont le rôle est capital dans la formation des principaux phénomènes atmosphériques. La pression barométrique, considérée autrefois comme l'élément météorologique principal, n'est plus qu'un élément à considérer parmi beaucoup d'autres, et la forme des figures isobariques n'est souvent plus qu'une indication secondaire.

Alors que la simple comparaison des parasites atmosphériques aux diverses figures isobariques, et, en particulier, aux diverses formes de dépressions, ne saurait nous amener à énoncer des relations bien précises, puisque l'un des éléments de comparaison, l'élément météorologique, n'est ainsi que très vaguement déterminé, la comparaison des atmosphériques avec les caractères spécifiques des masses d'air d'origine et de nature bien définies, leur comparaison aussi avec les mécanismes divers des diverses surfaces de discontinuité nous révéleront des relations très étroites entre les phénomènes radioélectriques et les phénomènes météorologiques.

Il importe donc, au seuil d'une telle étude, de rappeler les principaux concepts de météorologie moderne auxquels, nécessairement, il sera fait appel par la suite.

Les *perturbations météorologiques* se groupent en *courants* dont les *lits* contournent de vastes régions où la pression se modifie peu pendant d'assez longues périodes et que l'on nomme, après Teisserenc de Bort, *centres d'action* de l'atmosphère. Il y a des *centres d'action positifs*, où la pression est relativement élevée, ce sont les *anticyclones*. Il y a des *centres d'action négatifs* où la pression est inférieure à la moyenne, ce sont les vastes zones dépressionnaires.

Parmi les courants de perturbations qui contournent les anticyclones, qui contournent ou traversent les zones dépressionnaires, il faut de suite donner une importance particulière à ceux qui se rattachent à un ensemble que Bjerknes a nommé le *front polaire*. Le front polaire se déroule tout autour de la terre le long des latitudes moyennes. Il est le lieu de rencontre des masses d'air dites *polaires* provenant des régions polaires, et des masses d'air dites *tropicales* venant plus ou moins directement des tropiques. Bjerknes a montré que les surfaces de contact entre ces deux masses d'air peuvent être mises en évidence sur des cartes météorologiques suffisamment précises par des discontinuités de la direction et de la vitesse du vent, de la température, etc., d'où leur nom de *surfaces de discontinuité,* que les sondages aérologiques révèlent l'existence de ces discontinuités en altitude où elles sont presque toujours beaucoup plus nettes qu'au sol. On constate ainsi que la pente de ces surfaces est très faible (quelques centimètres) et que l'air froid plus lourd est toujours au-dessous (voir fig. 7, partie supérieure). Les surfaces de discontinuité se déplacent. Ce déplacement peut s'opérer de deux manières, suivant que c'est l'air chaud qui remplace l'air froid ou l'air froid qui remplace l'air chaud. Dans le premier cas, l'air chaud monte au-dessus de la surface, se refroidit par détente et provoque ainsi de longues pluies continues. C'est ce qu'on nomme un *front chaud.* Dans le second cas, l'air froid expulse l'air chaud en se glissant en coin sous la surface de discontinuité. C'est le phénomène du *front froid* accompagné le plus souvent d'averses, de grains et même d'orages.

Un *cyclone* du front polaire se développe le long de la surface de discontinuité dans les conditions suivantes : une langue d'air chaud pénètre dans la masse d'air polaire; un front chaud se développe ainsi et progresse. Cependant, derrière la langue d'air chaud, l'air polaire progresse à son tour, refoule en la soulevant la masse d'air chaud. La surface de séparation de ces deux dernières masses d'air porte le nom de front froid. Le front froid s'avance assez vite pour que sa trace sur le sol vienne rejoindre la trace du front chaud, *coupant* ainsi au sol la langue d'air chaud de la masse d'où elle émane (phénomène de la *séclusion*). Après la séclusion l'air froid continue à se glisser sous l'air chaud qui bientôt ne touche plus du tout le sol (phénomène de l'*occlusion*).

Plusieurs cyclones naissent ainsi successivement l'un derrière l'autre le long de la surface de discontinuité, formant des *familles.* Puis la masse d'air polaire qui vient fermer l'un des cyclones est assez abondante pour rompre le front polaire et venir inonder d'immenses surfaces de pays parfois jusque dans les latitudes tropicales. Ce sont les *invasions polaires,* phénomènes qui mettent fin à une famille de cyclones. Le front polaire est ainsi discontinu autour du globe.

Les masses d'air que séparent les surfaces de discontinuité ne sont pas toutes des masses d'air ascendantes, se soulevant ou soulevées le long de ces surfaces. Dans de nombreux cas, elles fuient au contraire au-devant de la surface ou encore la surface fuit sous elles. Alors les masses d'air s'étalent et s'affaissent, se réchauffant adiabatiquement en provoquant la résorption par évaporation des nuages et des brouillards. C'est ce qu'on nomme les surfaces *d'affaissement* ou de *subsidience* (Abgleitfläche signalées par Stüve). On les rencontre dans un très grand nombre de situations, en particulier en avant des fronts chauds, derrière les fronts froids, sur la face occidentale des anticyclones, etc.

Dans les invasions polaires, les masses d'air polaire ne sont pour ainsi dire jamais homogènes, mais se refroidissent par échelons; et l'on y rencontre très souvent, se suivant les uns les autres, plusieurs *fronts froids secondaires* dont la structure rappelle beaucoup celle des fronts froids proprement dits.

Scherechewsky et Wehrlé ont montré que les surfaces de discontinuité du front polaire ne sont pas les seules qui provoquent la naissance de perturbations. Partout où deux masses d'air différentes se trouvent en présence, se développent de *pseudo fronts polaires* et s'établissent des courants de perturbations météorologiques qui rappellent ceux du front polaire. L'un des courants de ce genre qui intéresse le plus directement l'Europe occidentale est celui qui naît sur la face sud-est de l'anticyclone des Açores entre les masses d'air échauffées par le Sahara et les masses d'air relativement froides de l'Atlantique. La France est souvent traversée par ce courant de perturbations qui l'aborde par le sud-ouest. En général alors, le gradient barométrique est très faible et c'est là la situation orageuse typique.

2. — Répartition générale des atmosphériques dans les centres d'action et les courants de perturbations.

A. *Centres d'action anticycloniques.* — A l'intérieur de vastes anticyclones, la gêne provoquée par les atmosphériques est faible en général. Les atmosphériques ne sont cependant pas toujours absents dans ces cas. Mais ils s'y manifestent d'une manière très régulière, apparaissent le soir, se maintenant pendant la nuit et disparaissant le matin. Ces atmosphériques nocturnes d'anticyclones paraissent d'autant plus nets que les ondes sont plus courtes et que la saison est plus froide. C'est ainsi que pendant trois hivers de suite, on a pu constater que les échanges de radiogrammes météorologiques sur ondes courtes entre la France et le *Jacques Cartier* (naviguant entre la France et le golfe du Mexique) ont été particulièrement brouillés à la fin des nuits au cours desquelles un anticyclone s'étendait sur la France. Il semble bien également qu'à ces époques les atmosphériques gênaient davantage les émissions sur ondes courtes que les émissions sur ondes longues.

B. *Courants de perturbations.* — Si les stations d'observation se trouvent placées dans le lit des courants de perturbations météorologiques le plus grand désordre semble régner dans la répartition des atmosphériques dans le temps et dans l'espace. Il serait entièrement inexact de dire que le fait de se trouver dans un courant de perturbations entraîne la présence d'atmosphériques nombreux et violents, il serait également inexact de dire que les courants de perturbations n'ont aucune action sur les atmosphériques. La vérité, c'est que les *diverses phases* d'une perturbation météorologique entraînent des états électriques entièrement différents les uns des autres, de même qu'elles se caractérisent par des états du ciel manifestant une très grande variété. Si l'on ne fait entrer en ligne de compte que les atmosphériques susceptibles de gêner fortement les réceptions radiotélégraphiques, on sera amené à considérer que certaines perturbations sont pauvres et que d'autres sont riches en atmosphériques.

Les cas de perturbations riches en parasites se partagent en deux groupes :

a) Dans le premier, les atmosphériques gênants apparaissent aussi bien le matin que l'après-midi ou que la nuit et ils ne s'établissent que *successivement* dans les différentes stations d'un même pays pour disparaître de même *successivement* dans ces stations. Ces périodes de brouillage coïncident toutes avec les phénomènes météorologiques connus sous le nom d'*invasions polaires*. Je rappelle qu'on observe ces phénomènes à la fin du passage d'une famille de cyclones. Une immense masse d'air polaire se précipitant à l'arrière du dernier des cyclones de la famille vient déchirer le front polaire, se répand vers le Sud, atteint quelquefois les tropiques, et déverse dans les contrées de latitude moyenne une masse d'air relativement froide parce qu'elle provient en ligne presque directe des régions polaires. Cette masse d'air est surtout froide en altitude puisqu'elle n'y a rencontré aucune cause de réchauffement rapide; par contre elle s'est réchauffée sensiblement au contact de la mer et au contact du sol dans les latitudes moins septentrionales que celles d'où elle provient. Alourdie en haut, allégée en bas, cette masse d'air devient instable; elle est par suite le siège de courants verticaux accusés. Nous avons déjà dit que ces courants étaient éminemment favorables à la formation d'atmosphériques forts;

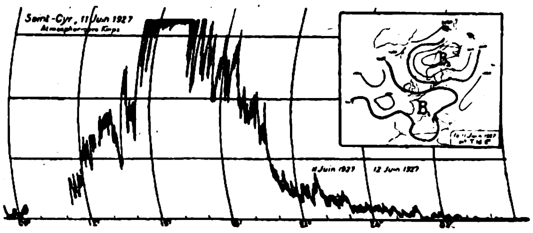

Fig. 2. — Variation diurne caractéristique des atmosphériques stagnants.

(Ces atmosphériques sont dus à l'action d'un pseudo front polaire venant du Sud-Ouest et auquel est dû en partie l'aspect de la carte isobarique et la dépression *B*.)

b) Le second groupe d'atmosphériques violents présente un tout autre caractère. Les atmosphériques sont partout localisés à certaines heures de la journée, ils débutent vers 11 h. 30 du temps local, ont leur maximum vers 15 heures et se prolongent jusque vers 21 heures et quelquefois plus avant dans la nuit. Mais, entre le lever du soleil et 11 h. 30 il règne un calme relatif, et très souvent on ne relate aucun brouillage dans cette période de la journée. Cette sorte de pulsation diurne s'observe simultanément dans tous les postes de la région soumise aux brouillages. Les atmosphériques y présentent simultanément une recrudescence vers midi et des affaiblissements à 21 heures (fig. 2). Ils peuvent être alors d'une rare violence et d'une continuité absolue; il arrive souvent qu'ils se prolongent comme un roulement ininterrompu pendant l'après-midi tout entière, rendant impossibles la plupart des réceptions et ne permettant que celles des émetteurs voisins ou puissants. Ce genre de brouillages, contrairement au précédent, n'intervient pas en toutes saisons. Il peut se constater en France à partir du mois d'avril, il semble particulièrement fréquent au mois de mai. Il ne semble pas qu'on l'observe entre le mois d'octobre et le mois d'avril. La répartition de ce genre d'atmosphériques à des heures fixes de la journée et le fait qu'ils apparaissent partout en même temps dans la région qui est affectée par eux, sans présenter de translation en surface, m'ont amené à les dénommer *atmosphériques stagnants* par opposition aux autres atmosphériques, dus, soit aux invasions polaires, soit aux fronts froids. J'ai donné à ces derniers atmosphériques le nom d'*atmosphériques migrateurs*.

En France, les situations météorologiques qui provoquent les *atmosphériques stagnants* sont celles au cours desquelles le pays est sous l'action de courants de perturbations orageux nés sur la face sud-est de l'anticyclone des Açores (voir fig. 2). Ces courants sont dus vraisemblablement au contraste de température entre l'anticyclone des Açores froid en altitude et les masses d'air africaines échauffées par le désert. Si, par un processus ou par un autre, l'anticyclone des Açores étend vers l'Est un prolongement sur la France, aussitôt le courant de perturbations météorologiques cesse de traverser le pays et aussitôt aussi disparaissent les atmosphériques stagnants. Tout se passe comme si l'anticyclone des Açores avait poussé un verrou barrant à la fois le chemin aux perturbations météorologiques et aux atmosphériques stagnants.

3. — L'action des perturbations météorologiques du front polaire.
Disparitions et recrudescences d'atmosphériques.

A. *Disparitions.* — Dans les latitudes moyennes l'arrivée d'un front chaud entraîne l'affaiblissement rapide des atmosphériques, quelquefois en moins d'une demi-heure. Ce phénomène est très sensible dans le cas d'un front chaud jeune, cas assez rare en France. Il l'est également dans le cas de fronts chauds en altitude, qui sont presque les seuls qu'on observe en Europe occidentale (cyclones occlus). Les noyaux de baisse barométrique, qui sont l'indice du passage d'une masse d'air chaud, au sol ou en altitude, s'accompagnent très souvent de la diminution ou de la mort des atmosphériques (fig. 3).

Ce phénomène d'affaiblissement et même de disparition est extrêmement général. Sous les latitudes moyennes, on l'observe aussi bien en été qu'en hiver, sur les continents que sur les océans (le *Jacques Cartier* l'a observé à chacune de ses traversées de l'Atlantique Nord), la nuit que le jour.

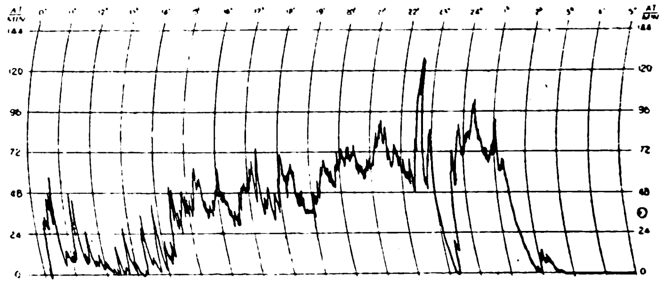

FIG. 3. — Diagramme d'atmosphériques du 21 octobre 1926.
(Saint-Cyr.)

(Exemple de disposition due à l'arrivée d'un front chaud en altitude.)

Non seulement l'arrivée d'un front chaud au sol ou en altitude est accompagné d'un affaiblissement de l'activité des atmosphériques migrateurs, mais elle a également un effet d'affaiblissement marqué sur les autres catégories d'atmosphériques et en particulier sur les atmosphériques nocturnes. Quand un front chaud commence à aborder un pays, les atmosphériques nocturnes peuvent disparaître presque complètement et même parfois totalement. Il en résulte que l'absence d'atmosphériques la nuit, qui est un fait anormal, est un très sérieux indice de l'arrivée prochaine du mauvais temps, surtout lorsqu'on était auparavant dans une période anticyclonique. Cette action des fronts chauds sur les atmosphériques nocturnes d'anticyclone a des conséquences importantes : elle prouve que les deux catégories d'atmosphériques, atmosphériques nocturnes et atmosphériques migrateurs en liaison avec le front polaire, bien que distinctes l'une de l'autre ne sont cependant nullement des phénomènes indépendants qui se superposeraient. Sinon l'action du front polaire pourrait ajouter quelque chose aux atmosphériques nocturnes, mais non point y faire des retranchements. D'autre part, cette action des fronts chauds sur les atmosphériques nocturnes montre également que ces derniers ont fatalement une origine peu éloignée, sinon ils ne pourraient être affectés par des phénomènes météorologiques rapprochés. On ne peut en effet attribuer cette disparition anormale à des phénomènes de propagation lointaine d'ondes électromagnétiques, puisque l'arrivée d'un front chaud ne réduit pas la portée nocturne des postes émetteurs lointains.

La faible activité ou même l'absence d'atmosphériques s'observe non seulement à l'approche d'un front chaud, mais également dans les secteurs chauds quand ceux-ci viennent jusqu'au sol. Ce phénomène météorologique est assez rare en Europe occidentale il a pu être observé à plusieurs reprises sur l'océan Atlantique Nord à bord du *Jacques Cartier*. Même dans les cyclones de basses latitudes le secteur chaud n'est le siège d'aucune activité des atmosphériques.

L'air du secteur chaud se refroidit dans ses couches basses au fur et à mesure qu'il recouvre des latitudes plus élevées; il devient très stable puisqu'il s'alourdit en bas, en restant léger en haut, alors que le phénomène exactement inverse se passe dans l'air polaire. Ce processus qui engendre la stabilité de l'air dans les invasions d'air tropical (comme le processus inverse engendre l'instabilité dans les invasions polaires) est vraisemblablement aussi celui qui provoque un calme électromagnétique presque absolu dans les secteurs chauds et une grande agitation électromagnétique dans les invasions polaires.

Les cas d'une stratification horizontale stable des masses d'air ne s'observent pas seulement pour les secteurs chauds. On les remarque également dans les surfaces d'affaissement ou surfaces de subsidience.

Dans les surfaces de subsidience, l'air s'affaisse soit parce qu'il descend le long d'une surface de discontinuité, soit parce que la surface de discontinuité fuit au-devant de lui; dans ce cas comme dans l'autre la subsidience entraîne une stratification stable *et une disparition d'atmosphériques*. C'est à un phénomène de ce genre qu'il faut très probablement attribuer la mort des atmosphériques en avant d'un front chaud et non pas au front chaud lui-même. C'est également à un phénomène de ce genre qu'il faut attribuer la disparition provisoire des atmosphériques qui se produit, comme nous allons l'indiquer plus loin, aussitôt après le passage des lignes de grains.

Les disparitions d'atmosphériques à la suite de phénomènes météorologiques ne s'observent pas seulement dans les phénomènes rattachés au front polaire. On les constate aussi dans des phénomènes météorologiques des tropiques et en particulier dans les typhons, comme l'a signalé Gherzi. Les typhons se produisent dans des régions où les atmosphériques violents sont la règle absolue. Or quand un typhon s'approche d'un poste récepteur ou passe à proximité de lui, les atmosphériques s'affaiblissent très sensiblement et sont entièrement absents au centre du météore.

Pour en terminer avec cette question de la disparition des atmosphériques par suite de phénomènes météorologiques, je tiens à attirer l'attention sur une opinion assez répandue et qui est que les cyclones fabriquent des atmosphériques en leur centre et que, grâce au radiogoniomètre, on peut aussi déceler le centre de ces météores même à très grandes distances. Ce que nous venons de voir nous montre que c'est là une erreur. Tout au plus peut-on déterminer ainsi l'approche et la proximité de fronts froids bien marqués. Je vais maintenant étudier l'action électromagnétique de ces phénomènes météorologiques connus sous le nom de fronts froids.

B. *Apparition et renforcement des atmosphériques.* — Dans les latitudes moyennes, les fronts froids entraînent une augmentation ou une apparition des atmosphériques. Le phénomène est général, bien qu'il puisse se produire avec des intensités très diverses. Dans certains cas un front froid bien caractérisé produit une telle recrudescence d'atmosphériques que toutes les réceptions radiotélégraphiques en deviennent très pénibles. Dans d'autres cas au contraire les atmosphériques provoqués par le passage de la discontinuité météorologique sont trop faibles pour brouiller des émissions même peu puissantes, et on ne remarque le passage qu'à condition de le surveiller particulièrement. Même dans ces cas, l'influence du front froid sur le nombre et l'intensité des atmosphériques est indéniable.

Il faut rattacher l'action des fronts froids à celle des invasions polaires qui a été décrite plus haut. Les atmosphériques de fronts froids sont essentiellement des atmosphériques migrateurs qui se propagent en même temps que ces fronts. Le mécanisme de leur formation est identiquement le même que celui de la formation des atmosphériques d'invasion polaire. On peut même dire que les atmosphériques d'invasion polaire ne sont qu'un cas particulier des atmosphériques de front froid (fig. 4 et 5).

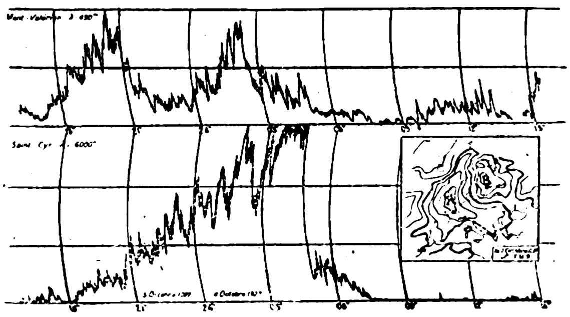

Fig. 4. — Enregistrements d'atmosphériques dus à l'air polaire.

On remarquera que, dans ce cas, l'évolution des atmosphériques peut être différente même dans deux localités peu éloignées (ici 20 km. de distance entre les deux récepteurs). Cependant la différence peut être due ici en partie aux longueurs d'onde (480 mètres d'un côté et 6,000 de l'autre.)

On constate, surtout en été, des discontinuités de front froid extrêmement accentuées qui apparaissent en général en France au sud-ouest du pays; la ligne de discontinuité s'étend sur plusieurs centaines de kilomètres de longueur et traverse peu à peu la France du Sud-Ouest au Nord-Est. Ces discontinuités sont accompagnées de parasites atmosphériques intenses, même lorsqu'elles ne donnent pas encore naissance à des orages, ni à des grains, ni à des précipitations, ni même à des formations nuageuses. Ceci montre bien que les atmosphériques de fronts froids ne sont pas une conséquence secondaire de ces fronts qui les provoqueraient par l'intermédiaire d'orages, de nuages ou de précipitations. Ils sont au contraire un phénomène primaire qui évolue parallèlement aux phénomènes météorologiques provoqués par le front froid et qui peut même se produire sans eux.

L'action des fronts froids du front polaire peut s'étendre très au Sud, jusque dans les régions tropicales, dans certaines conditions favorables. Certaines perturbations du front polaire peuvent se prolonger dans ces régions sous forme de fronts froids en altitude. On ne les met en évidence sur les cartes météorologiques que par certaines perturbations dans le régime des vents et leur passage se dénote souvent par des crochets brusques de la température et de l'humidité, par l'apparition de certains systèmes nuageux, quelquefois à peine indiqués et d'autres fois très vigoureux et donnant des météores violents. L'arrivée de ces fronts froids est annoncée plusieurs centaines de kilomètres à l'avance par une recrudescence très sérieuse des parasites atmosphériques qui, dans ce cas, sont, sous les basses latitudes, le seul élément météorologique susceptible de signaler localement et à l'avance l'existence d'une perturbation météorologique.

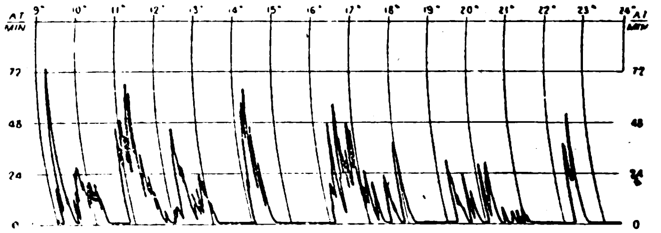

Fig. 5. — Diagramme d'atmosphériques du 15 novembre 1926, à Paris.

(Autre exemple d'atmosphériques dus à l'air polaire.)

Quelle est *la portée des atmosphériques de fronts froids*? L'expérience montre qu'elle peut être très variable (plusieurs centaines de kilomètres à quelques centaines de mètres).

D'après des observations faites à bord du

Jacques Cartier sur l'Atlantique il semble bien que ces différences de portée doivent être attribuées à la constitution physique des masses d'air; quand nous avons affaire au front froid d'un cyclone jeune et que les deux masses d'air voisines n'étant en présence que depuis peu de temps possèdent encore tous leur caractères propres, les atmosphériques n'apparaissent pas avant le front froid, mais aussi tôt que lui et quelquefois un peu après. Au contraire, quand la discontinuité de fronts froids est ancienne, la recrudescence des atmosphériques commence plusieurs centaines de kilomètres avant le passage du front.

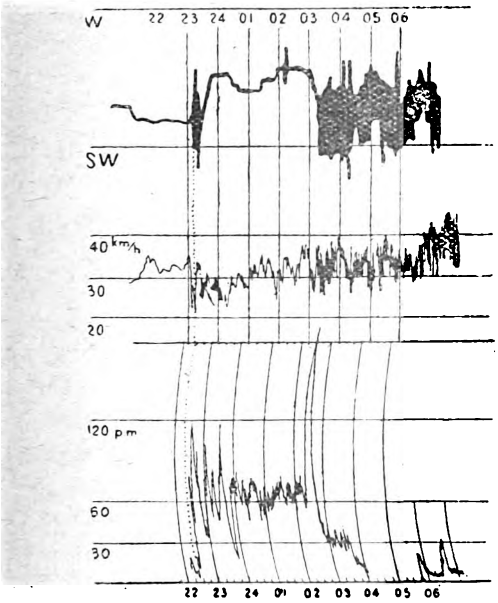

FIG. 6. — Exemple de simultanéité dans les variations de vent et les variations des atmosphériques (8 octobre 1926).

Courbe supérieure : Direction du vent au sommet de la Tour Eiffel.
Courbe intermédiaire : Vitesse du vent au sommet de la Tour Eiffel.
Courbe inférieure : Nombre par minute d'atmosphériques au Mont Valérien.

(L'augmentation brutale des atmosphériques à 22 heures, la pointe à 2 h. 30 coïncident avec le remplacement d'une masse d'air par une autre. Le décalage entre les heures au Mont Valérien et les heures à la Tour Eiffel correspond au temps mis par les surfaces de discontinuité pour aller d'un point à l'autre.)

En *Europe* la recrudescence des atmosphériques se produit à des distances qui varient entre 50 et 150 kilomètres avant le passage du front.

En *Afrique occidentale française* la recrudescence des atmosphériques se produit à des distances de 300 kilomètres pour des fronts froids *orageux*.

Les atmosphériques qui précèdent et annoncent le front froid ne sont probablement pas, tout au moins pour la plus grande part, comparables à une émission du front froid qui serait perceptible à distance. Ils ne prennent pas naissance dans le front froid lui-même, mais *dans l'air qui précède le front.* Ceci s'indique très nettement sur les enregistrements d'atmosphériques. On constate en effet que, loin de croître régulièrement jusqu'au passage du front froid comme cela se passerait s'ils en émanaient, les atmosphériques apparaissent brusquement à une certaine distance, puis présentent des variations très sensibles allant quelquefois jusqu'à s'annuler pour subir un violent renforcement au passage du front lui-même. D'autre part les enregistrements parallèles des phénomènes météorologiques et des parasites atmosphériques révèlent toujours dans le cas du temps de traîne ou du temps à grains une correspondance étonnante dans l'évolution des deux phénomènes (fig. 6).

Il ressort de tout ceci que la plupart de ces atmosphériques sont provoqués par le caractère purement local des masses d'air et que leur action s'atténue assez vite avec la distance pour qu'ils cessent d'être sensibles à quelques centaines de mètres du lieu de leur naissance, ce qui ne les empêche pas d'être parfois d'une rare violence et assez nombreux pour provoquer dans les réceptions les brouillages les plus graves.

Les fronts froids ne produisent pas seulement un renforcement des atmosphériques. Aussitôt après leur passage, on constate presque toujours une brusque diminution de ceux-ci et souvent même une disparition presque totale pouvant durer plus d'un quart d'heure. La bande de disparition d'atmosphériques qui fait suite à la bande de recrudescence et qui se promène à travers le pays en même temps que la ligne de grains, montre encore à quel point toute cette catégorie d'atmosphériques a un caractère nettement local (fig. 7).

Il est cependant certains fronts froids particulièrement intenses et accompagnés en général d'orages violents, dans lesquels on a constaté l'existence d'atmosphériques ayant une portée sensible dépassant certainement plusieurs centaines de kilomètres et quelquefois davantage. En Angleterre, M. Watson Watt s'est attaché à la recherche de tels atmosphériques. Il signale des cas où, à l'aide de procédés radiogoniométriques, il a pu mettre en évidence l'action des atmosphériques provoqués par des fronts froids situés à plusieurs milliers de kilomètres.

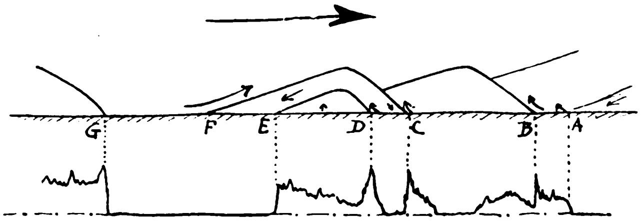

Fig. 7. — Schéma de l'évolution des atmosphériques dans le front polaire.

En haut, coupe verticale à travers les différentes masses d'air; en bas, atmosphériques.

En A, apparition des atmosphériques annonciateurs du front froid de trace B.

En C et D, maxima d'atmosphériques au passage de lignes de grains.

En E, disparition des atmosphériques en avant du pont chaud de trace F.

En G, apparition brutale d'atmosphériques coïncidant avec l'arrivée de l'air polaire succédant à l'air tropical du secteur chaud F G.

Chaque fois qu'on a cru mettre en évidence l'existence d'atmosphériques provenant du « Centre » d'une dépression on n'a constaté en fait que l'action classique des fronts froids.

4. — Les courants de convection et le gradient vertical de la température.

J'ai déjà signalé plus haut le contraste entre la *stabilité verticale de l'air tropical* des fronts chauds correspondant à une disparition d'atmosphériques et l'*instabilité verticale de l'air polaire* correspondant à une recrudescence de ceux-ci. Cette relation entre l'instabilité verticale de l'air et l'activité des atmosphériques est absolument générale et semble pouvoir expliquer la formation de toutes les catégories d'atmosphériques. A l'origine du caractère météorologique de chaque masse d'air, on trouve le mode de répartition verticale des températures dans cette masse. Suivant que la décroissance de la température avec l'altitude est plus ou moins rapide, l'air est plus ou moins instable verticalement. Toutes les propriétés météorologiques des masses d'air, et en particulier l'état du ciel, les nuages, les précipitations, dépendent directement du degré de stabilité ou d'instabilité verticale de l'air. De même *l'existence ou l'absence des parasites atmosphériques sont des conséquences de cette stabilité ou de cette instabilité verticale.*

L'instabilité des masses d'air peut avoir diverses causes auxquelles correspondent les diverses catégories d'atmosphériques. Quand l'instabilité est provoquée par le transport horizontal de masses d'air, c'est-à-dire à une origine dynamique (c'est le cas des perturbations au front polaire), nous nous trouvons en présence *d'atmosphériques migrateurs* (atmosphériques observés dans les invasions polaires et au passage des fronts froids). Le processus de cette instabilité a été décrit plus haut.

Dans certaines situations météorologiques en certaines saisons et sur les continents, l'échauffement des basses couches de l'atmosphère par l'intermédiaire d'un sol surchauffé par le soleil peut provoquer de violentes instabilités et des courants de convection intense. A cette instabilité correspondent les *atmosphériques stagnants* qui apparaissent vers midi et sont particulièrement violents vers 15 heures au moment où la convection est le plus accentuée.

La nuit, le refroidissement par rayonnement des basses couches de l'atmosphère provoque la formation d'une couche d'inversion, à la surface de séparation entre l'air plus chaud supérieur et l'air refroidi et lourd des basses couches. Le mouvement horizontal des masses d'air supérieur provoque dans la couche sous-jacente une instabilité tourbillonnaire à laquelle correspondent les atmosphériques nocturnes. L'existence de ces couches d'inversion est particulièrement sensible la nuit dans les anticyclones, par suite du rayonnement plus intense sous le ciel pur et par suite de l'absence des perturbations dynamiques capables de balayer l'instabilité. C'est pourquoi les atmosphériques nocturnes sont particulièrement nets dans les périodes anticycloniques, à tel point que j'avais été amené à les appeler *atmosphériques d'anticyclone.*

Comme l'instabilité verticale de l'atmosphère est un des éléments déterminants de l'aspect des phénomènes météorologiques et comme son observation directe à l'aide de sondages est onéreuse et parsemée, on voit toute l'utilité qu'on peut espérer tirer, au point de vue météorologique, de l'observation des atmosphériques, puisque cette observation fournira l'un des procédés les plus simples et les plus économiques pour obtenir des indications sur l'instabilité verticale de l'atmosphère.

5. — Évolution parallèle des atmosphériques et des individus météorologiques.

Nous sommes maintenant en état de comparer d'une façon logique l'évolution des parasites atmosphériques et l'évolution des individus météorologiques isolés. Nous verrons pourquoi, chaque fois que l'on veut commencer l'étude par cette comparaison brutale, on aboutit à des résultats contradictoires, soit à des conclusions inexactes :

a) *Les nuages.* — Les fronts chauds comme les fronts froids provoquent des formations nuageuses. Il est donc évident que certains temps couverts correspondront à une absence ou à une disparition d'atmosphériques et que d'autres correspondront à une recrudescence de ceux-ci. Une comparaison brutale de la nébulosité et des atmosphériques ne donnera donc aucun résultat. Par contre il y a une liaison très intéressante à établir entre les formes de nuages et les armosphériques. Comme les formes de nuages sont déterminées pour une très grande part par la structure verticale de l'atmosphère, les formes bourgeonnantes et tourmentées correspondent à une grande instabilité et en général à une assez grande activité des atmosphériques, les formes aplaties et stratifiées correspondent très souvent à l'absence ou à la faiblesse des atmosphériques. De même, les cas de ciel pur peuvent correspondre à l'absence ou à la présence d'atmosphériques.

b) *Brouillards.* — Dans l'ensemble l'activité des atmosphériques augmente quand la visibilité s'améliore, ceci par suite de l'influence des temps de traîne dans lesquels les mouvements de convection nettoient l'atmosphère de toutes poussières et provoquent en même temps des atmosphériques nombreux. Par contre il est certaines situations dans lesquelles des atmosphériques sont observés par brouillard et disparaissent en même temps que le brouillard. Il faut probablement rattacher ces situations au cas des atmosphériques d'anticyclone.

c) *Précipitations.* — On peut répéter à ce sujet ce qui a été dit pour les nuages.

d) *Tempêtes et cyclones.* — Une légende accuse les tempêtes et les cyclones d'être un nid d'atmosphériques. Aussi a-t-on cherché souvent à en déceler l'approche à l'aide d'un radiogoniomètre qui indiquerait à la fois la direction des atmosphériques et les centres de ces météores. Tout ce que nous venons de voir montre que, non seulement cette relation est inexacte, mais qu'elle n'a à proprement parler aucun sens, surtout pour les perturbations des régions tempérées (front polaire) dans lesquelles on distingue plusieurs phases complètement différentes. Chacune des phases joue un rôle particulier dans l'évolution des parasites atmosphériques; il suffit de se reporter à ce qui a été dit plus haut. Quant aux météores tropicaux, nous avons vu que le centre des typhons, loin d'être une usine d'atmosphériques, est l'endroit le plus calme qu'on puisse souhaiter pour assurer des réceptions radiotélégraphiques sans être gêné par eux. En ce qui concerne les cyclones des Antilles, je ne connais aucune observation précise d'atmosphériques faite en leurs centres, mais au contraire une observation de disparition d'atmosphériques faite à bord du *Jacques Cartier*.

e) *Grains et orages.* — Pour les grains, orageux ou non, il suffit de se reporter aux numéros précédents. Quant aux orages, une petite mise au point est nécessaire. Il est indéniable qu'un grand nombre d'éclairs sont accompagnés d'atmosphériques, mais il est non moins certain que ces atmosphériques ne forment qu'une infime partie de ceux qu'on observe même en période orageuse. Ceux-ci sont particulièrement intenses *avant* l'arrivée des orages, alors même qu'on n'observe aucun éclair; ils diminuent souvent pour disparaître parfois presque totalement *pendant* que l'orage éclate, et presque toujours ils sont très faibles ou inexistants *quand l'orage s'est éloigné* de la station d'observation. Les seuls que l'on observe alors sont

ceux qu'accompagnent les éclairs que l'on voit apparaître à l'horizon, c'est-à-dire qu'il en existe très peu et que les réceptions radiotélégraphiques même faibles ne sont nullement brouillées.

* * *

Measurements of the effective heights of the conducting layer and the disturbances of August 19, 1927 (1)

by O. DAHL and L. A. GEBHARD,
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General Method. — The experiments described below were carried out during the summer of 1927. The method is that described by Breit and Tuve (2). As before, the signals were transmitted at the Naval Research Laboratory and received at the Department of Terrestrial Magnetism.

The experimental arrangements were almost identical with those employed by Breit and Tuve. The general scheme was as follows : A crystal-controlled, 4,015-kilocycle, 10-kilowatt set located at Bellevue was modulated so as to send out short interrupted trains of waves. The duration of each train was roughly 1/1,500 second, the space between the end of one train and the beginning of another being about 1/750 second. The signals were received on the roof of the main building of the Department of Terrestrial Magnetism. The receiving aerial was a small loop. This was connected to the first detector of a superheterodyne set having an intermediate frequency of 50 kilocycles. The second detector of the superheterodyne was fed into a resistance of 25,000 ohms connected between the filaments and grids of four 7.5-watt tubes (n° 210 x) used in parallel. The output of these tubes was passed through a General Electric oscillograph with a proper balancing arrangement for the steady plate-current. The resultant wave-form was photographed and observed visually.

The idea of the method is to photograph on the same record the trains arriving directly over the ground from Bellevue as well as their echoes arriving *via* the conducting layer. The lag between the arrival of the echo and the train which produced it gives the effective height of the layer.

Transmitter. — Figure 1 shows a diagram of the transmitter. A quartz crystal-oscillator was used as a master. Its output was first amplified by a 250-watt tube and then by a

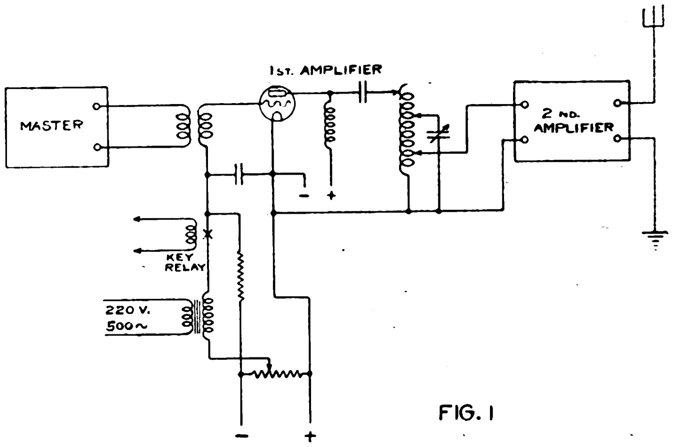

FIG. I

Fig. 1. — Diagram of transmitting circuit, crystal control, 10 kilowatts, 4,015 kilocycles.

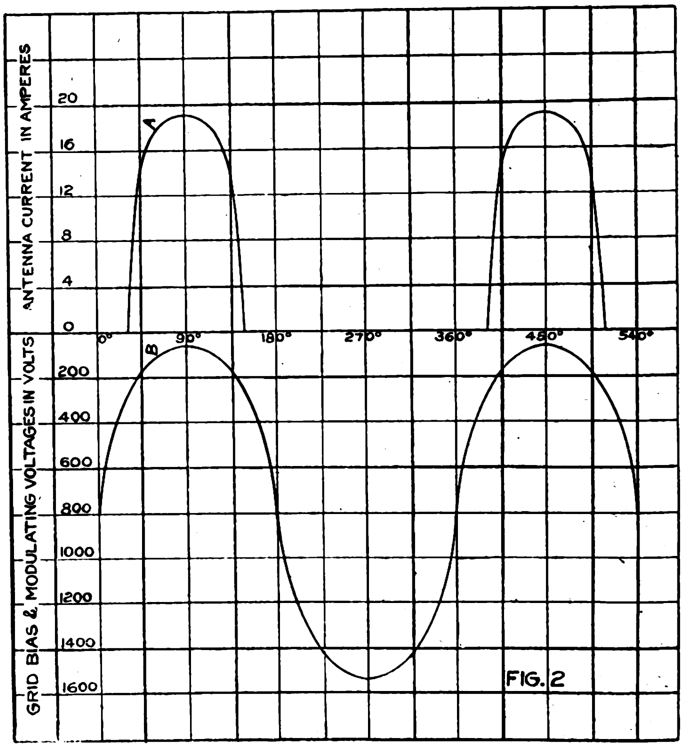

FIG. 2

Fig. 2. — Wave-form of transmitter as used in experiments.

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(1) Presented before the convention of the International Union of Scientific Radiotelegraphy, October 14, 1927.

(2) G. Breit and M. A. Tuve (*Phys. Rev.*, vol. 28, 1926, pp. 554-575).

20-kilowatt tube. Control and modulation were accomplished in the first, i. e., the 250-watt stage of the amplifier. In controlling, with the key up, a high negative voltage was placed on the grid. This stopped the amplifying action of the 250-watt tube. The same principle was used in modulating.

The modulation voltage was obtained from a 500-cycle generator fed through a transformer. The 500-cycle output of the transformer was passed through a voltage divider, and a proper part of the grid biasing voltage was superposed on a constant negative bias. Figure 2 shows the actual relations. The constant negative voltage was 800 volts. The alternating 500-cycle voltage had an effective value of 525 volts. Its graph against time is shown on curve (*b*). The portions of time in which the 250-watt tube is operative are shown on (*a*). The humps shown on (*a*) represent approximately the envelope of the antenna current.

Figure 3 shows a photograph of the transmitting apparatus.

Receiving Apparatus. — The difference between the receiving apparatus used in these tests and in those of Breit and Tuve consists in the following items :

1) Different location of the apparatus. The measurements were made on the roof of the main building, while in the summer of 1925 they were made in the small Experiment Building.

2) The receiving aerial was a small loop 2 feet square instead of an antenna.

3) 7.5-watt tubes were used in the power amplifier instead of the 5-watt tubes used previously. Four tubes in parallel were used in each case.

4) A 7.5-watt oscillator loosely coupled to the superheterodyne was substituted for the 201 A oscillator used previously.

Results. — The results obtained are summarized in the following table.

Determination of Heaviside-Layer Heights

DATE	TIME	HEIGHT IN MILES	TRANSMITTER
1927	h. m. h. m.		
Aug. 15	2 00 — 2 15 p. m.	128 or 66	Antenna
16	10 00 — 10 15 a. m.	70 or 124	Antenna
16	2 00 — 2 15 p. m.	62	Antenna
17	10 00 — 10 15 a. m.	60 and perhaps 110	Antenna
17	2 00 — 2 15 p. m.	53	Antenna
19	. . . .	No reflection	Antenna
22	2 00 — 2 15 p. m.	112 or 80	Antenna
23	10 00 — 10 15 a. m.	103	Horizontal doublet
23	10 30 — 10 45 a. m.	104	Antenna
23	2 00 — 2 15 p. m.	104	Antenna
23	2 30 — 2 45 p. m.	124	Horizontal doublet
24	10 00 — 10 15 a. m.	92	Horizontal doublet (ground wave stronger than reflected)
25	10 15 — 10 30 a. m.	103	Horizontal doublet (reflected wave stronger than ground)

It must be noted also that not only did the reflected wave disappear on August 19 but also that the « ground » wave itself was received with difficulty. During the same period the reception of signals at the Naval Research Laboratory was decidedly below the average — some familiar stations not coming in at all.

Reports were received that the abnormal conditions caused a great deal of difficulty in copying on the San Francisco-Washington high-frequency circuit both day and night. For three days previous to and including the 21st much trouble was had in clearing traffic to London, it being the first time in over a month that London did not receive everything on the first transmission.

A report from London stated that during this period poor receiving conditions existed on all short-wave stations to the west of London, but that conditions were normal in receiving stations from other directions. Another London report stated that the same trouble was experienced with the Canadian Beam. The conditions, however, apparently did not affect the reception of Washington's 12,045 kilocycles frequency in San Francisco. On August 19, Lakehurst, New Jersey, was unable to receive Washington's 8 : 15 a. m. weather broadcast on 4,015 kilocycles, and Pensacola, Florida, could not copy this broadcast on 8,030 kilocycles. The failure of these two stations to get the broadcast was considered unusual, and could not be accounted for since the weather broadcast had been properly transmitted.

Several people reported to the Naval Research Laboratory that during the disturbance broadcast signals in the 500-1,500 kilocycles band also showed abnormalities.

We attempted to ascertain whether this unusual condition was definitely connected with a visible disturbance on the sun. No sufficiently definite information could be obtained because, even though there was an unusual spot in the southwestern quadrant of the sun, it had been active several days before August 19.

However, our information seems fairly definite to the extent of indicating that just before the disturbance the layer height was around 50 miles, while after the normal condition had been reached again the layer height was increased to 100 miles and more.

Figures 4 and 5 give reproductions of typical wave-forms observed during this period. The first of these was taken on August 25 and the second on October 8. Figure 6 is a reproduction of the photograph of the sun on August 19 kindly put at our disposal by S. B. Nicholson of the Mount Wilson Observatory.

* * *

International Comparison of Frequency Standards,

by J. H. DELLINGER,

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This paper is a brief resume of the international comparisons of frequency standards in which the « Bureau of Standards » has participated in the past four years. The accuracy of such intercomparisons has steadily increased during this period, keeping pace with the increasing demands of practice. Those demands now require measurements of very great precision.

International comparisons of any physical quantity can be made by sending the standards of that quantity successively to the laboratories of the different nations. In the frequency comparisons that method has been followed with one exception. About four years ago it appeared that greater accuracy might possibly be obtained by another method, namely, the simultaneous measurement in the various laboratories of the frequencies of waves transmitted from stations of sufficient power to be received simultaneously in many nations. In 1924 such a set of comparisons was organized by the « Bureau of Standards » and measurements were made in the national laboratories of England, France, Italy, Germany and the United States. The results of such comparisons extending over a number of weeks were only fairly satisfactory. The accuracy of measurement was not as good as had been hoped. The result, briefly, was to show an agreement between the various countries within about two parts in 1,000. The methods used, and independent evidence of various kinds, were such that it was certain that the standards used in each of these countries were more accurate than this. The observed differences were therefore due in large part to the methods of measurement.

From the foregoing experience it was concluded that the more common method (actual transportation of standards from one laboratory to another) used in connection with other physical quantities should be considered anew. Just at that time, about three years ago, enough had been learned of the possibilities of piezo oscillators and piezo resonators to make it appear that they offered particular advantages as portable standards. Measurements upon piezoelectric devices therefore offered a method which might be superior to the scheme of measuring simultaneously the frequencies of transmitted waves. The first attempt of this kind was an informal series of measurements made in several national laboratories by Prof. W. G. Cady, Professor at Wesleyan University, Middletown, Connecticut. He took a number of piezo resonators (not oscillators) abroad; the results of his measurements are published in his article, « *An international comparison of radio wave length standards by means of piezoelectric resonators* » (1).

In December, 1925, the « Bureau of Standards » sent a piezo oscillator to Europe, sending it first to the « National Physical Laboratory » of England which was to send it in turn to General Ferrié in France, to Prof. Vallauri in Italy, to the « Physikalisch-Technische Reichsanstalt » in Germany, and then to be returned to the Bureau. A second piezo oscillator was sent in July, 1926, in the other direction, i. e., beginning with the « Physikalisch-Technische Reichsanstalt ». It is to be noted that complete piezo oscillators were sent, not merely the quartz plates.

The quartz plate is a standard of radio frequency having tremendous possibilities, but it is not a standard of absolute constancy as was assumed in unwarranted fashion by many people at the time that its possibilities in radio were first recognized. There are a number of factors which introduce variations in the frequency of the electromotive force produced by a vibrating quartz plate, and also in the frequency of response of a piezo resonator. These factors are the temperature, the particular method and details of the mounting of the quartz plate, the circuit constants of the associated circuits, and the methods of coupling between the circuit in which the quartz plate is inserted and the necessary auxiliary and measuring circuits. It is not true that highly accurate comparisons can be made by sending a quartz plate in the mail from one laboratory to another. The effects of the circuit constants may vary the frequency produced by a piezo oscillator several parts in 1,000, and therefore a complete piezo oscillator must be sent for accurate comparisons.

The measurements upon the two piezo oscillators sent to Europe by the « Bureau of Standards », on account of the necessary delays in getting from one laboratory to another, have only recently been completed. The results are very satisfactory, showing that the use of portable piezo oscillators is a far more dependable method of making international comparisons of frequency standards than simultaneous measurements of station frequencies. The differences between the measurements in the various laboratories have a maximum of eight parts in 10,000; the net result is to show an agreement by this method which may be summarized as a few parts in 10,000. It is uncertain as a result of this work as to whether the observed differences were due to variations in the piezo oscillators used as the means of comparison or to actual differences in the basic standards of the different countries.

The same method was used, namely the sending of piezo oscillators, to make comparisons between the frequency standards of the United States, Canada and Japan. In February, 1927, a piezo oscillator was sent to Canada, was there measured by the « Radio Service of the Department of Marine », then returned and measured again by the « Bureau of Standards ». The results were of the same order as those obtained in the European comparisons, and were considered by the Canadian administration as giving a comparison of standards to as great an accuracy as their measurements permitted.

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(1) *Proc. I. R. E.*, vol. 12, p. 805; December 1924.

The « Bureau of Standards » sent a piezo oscillator in March, 1927, to Japan. I have just received the results of the Japanese measurements from Prof. Yokoyama. The average of the Japanese values differs from the « Bureau of Standards » measurements by only one part in 10,000.

In February of this year a group of American commercial companies, the « General Electric C° », the « American Telephone and Telegraph C° », « Westinghouse C° », « Radio Corporation of America », « General Radio C° », and two departments of the government, the Navy and the « Department of Commerce », had a conference at the « Bureau of Standards », and drew up a program with a view to establishing a more accurate basis of frequency measurements. Each of these organizations had been conducting considerable research on this subject, and it seemed that progress could be made faster by combining the resources and experience of all the organizations. Incidental to the beginning of this program of work the « General Electric C° », « Westinghouse C° », and the Navy constructed a number of improved piezo oscillators. These are an advance over those previously used in that there is a thermostat provided to keep the temperature of the quartz plate constant.

As I was going to Europe this summer for the « Bureau of Standards » an opportunity was presented to use one of these instruments for comparisons with the national standards of the national laboratories of Europe. It was hoped that by taking this improved instrument to the European laboratories a frequency comparison could be obtained that would surpass previous ones in accuracy. I accordingly had the pleasure of making frequency measurements, all of which were very satisfactory, at the « National Physical Laboratory » in England, at the laboratory of the « Télégraphie militaire » under General Ferrié, in Paris, at the « Italian Navy Laboratory » in Livorno, Italy, and at the « Physikalisch-Technische Reichsanstalt », in Berlin. I wish to acknowledge the splendid cooperation given me in each of these laboratories by my collaborators, D^r^ D. W. Dye at the « National Physical Laboratory », Prof. R. Jouaust, in Paris, Prof. G. Vallauri, in Livorno, and D^r^ E. Giebe at the Reichsanstalt. The measurements were made in July and August.

We measured the frequencies of the piezo oscillator at the « Bureau of Standards » before I left the United States and again after my return. The instrument contained two quartz plates in a thermostatically controlled heated enclosure, the quartz plate being kept at a temperature of 46° C. Instruments were provided to insure that filament and plate voltages were always the same. The frequencies of both quartz plates were approximately 200 kilocycles.

It was interesting to find that in all the laboratories the same general method is used for frequency measurements of high precision. Typically, there are three generators placed in the laboratory about ten feet apart; one is the piezo oscillator under measurement, one is the auxiliary oscillator or heterodyne, and the third is the standard instrument against which the piezo oscillator is to be measured. There is coupled to each of these a single circuit which contains a receiving set with telephone receivers connected to it. The method of measurement is in all cases some variation of the simple procedure of listening for the frequency difference between the auxiliary generator and the piezo oscillator, and by one process or another reducing this difference to zero, and then adjusting to equality with the auxiliary generator the standard in terms of which the piezo oscillator is being measured (or else determining the difference between the standard and the auxiliary generator).

The results justified every hope. The differences between the different laboratories in these measurements ranged from zero to five parts in 100,000, the average of the departures from the mean being three parts in 100,000. This agreement is indeed as good as the degree of certainty of the national standards. It is concluded that by using a portable temperature-controlled piezo oscillator in which the currents through the circuits are always adjusted to the same value, it was possible to get a 10-fold increase of accuracy over the use of simpler types of piezo oscillator as in the previous comparisons.

This is not by any means the limit of attainable accuracy. The instrument with which these comparisons were made was one upon which complete studies had not been made as to temperature equilibrium, frequency lag, and temperature coefficient of the quartz plate. No doubt as time goes on we can improve upon this and arrive at our goal, which has been set as an accuracy of one part in 100,000. On the whole, however, I feel that it has been demonstrated that the several national laboratories are measuring frequencies with an accuracy very satisfactorily in advance of the immediate requirements of radio practice. Those requirements are becoming increasingly rigorous, and the national laboratories must therefore continue their efforts for even higher accuracy of frequency measurement.

* * *

L'emploi de cellules photoélectriques, associées à des lampes à plusieurs électrodes, à la solution de divers problèmes concernant la mesure du temps.

par G. FERRIÉ et R. JOUAUST,
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La mesure des intervalles de temps présente un intérêt primordial dans un grand nombre de problèmes scientifiques concernant la radiotélégraphie, en particulier pour la détermination des fréquences, qui nécessite une très grande précision. De plus, l'emploi de la radiotélégraphie pour l'émission de signaux horaires, et l'utilisation de ces derniers pour les études relatives à la propagation des ondes, aux origines des perturbations atmosphériques (statics), etc., augmentent encore l'importance que présente la mesure du temps pour les spécialistes de la radiotélégraphie.

Il n'y a pas lieu d'entrer ici dans les détails des méthodes et appareils établis jusqu'à maintenant pour la détermination et la conservation de l'heure, ainsi que pour la mesure des intervalles de temps. Nous nous bornerons à indiquer quelques dispositifs que nous avons créés en utilisant les cellules photoélectriques au potassium associées à des lampes à plusieurs électrodes et à des amplificateurs employés dans la technique courante de la radiotélégraphie (1).

Aménagement des pendules astronomiques. — Les pendules astronomiques, utilisées dans les observatoires pour conserver l'heure déterminée par les observations astronomiques, sont parvenues à un très haut degré de perfectionnement. Leur marche journalière est très régulière et très faible, et n'est généralement pas supérieure à un centième de seconde. On peut donc mesurer de longs intervalles de temps avec une très grande précision.

En revanche, quand on compare les durées des diverses secondes données par ces pendules, on s'aperçoit qu'elles diffèrent entre elles de quantités notables atteignant parfois le centième de seconde. Ces pendules ont donc une précision insuffisante quand on se sert de leur intermédiaire pour mesurer de courts intervalles de temps, d'une durée de quelques secondes par exemple. Les causes de ces irrégularités sont les suivantes :

Les pendules des observatoires, étant placées loin des lieux d'observation, souvent même dans des caves, ne sont pas sous les yeux des astronomes et, pour permettre d'utiliser leurs indications, on emploie le procédé ci-après :

La pendule porte un dispositif qui toutes les secondes (le plus souvent toutes les deux secondes) ferme un circuit électrique. La fermeture de ce circuit donne passage à un courant qui actionne la plume d'un chronographe ou produit un « top » dans un téléphone. Le dispositif de fermeture est constitué par un cliquet porté par le balancier et engrenant avec une roue dentée. On conçoit que quelques précautions qui aient été prises pour la taille des roues, leurs dents ne sont pas identiques.

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(1) Nous avons entrepris ces travaux dès 1922 et leurs résultats ont été successivement communiqués aux réunions des Unions internationales d'Astronomie et de Géodésie-Géophysique à Rome en 1922, Madrid 1924, Cambridge 1926, et publiés également dans des revues astronomiques et radiotélégraphiques ainsi que dans les Comptes rendus de l'Académie des Sciences.

La fermeture du circuit n'a donc pas lieu à l'instant précis où le balancier passe par sa position d'équilibre, et la quantité dont elle en diffère varie avec la dent qui engrène avec le cliquet. La première idée qui vient à l'esprit pour éliminer cette cause d'erreur consiste à chercher un procédé pour observer directement le balancier.

Le principe du dispositif de ce genre que nous avons établi dès 1923 pour l'Observatoire de Paris était le suivant :

Un miroir était fixé sur le balancier et, chaque fois que le balancier passait par la position d'équilibre, les rayons lumineux émanés d'une lampe électrique étaient réfléchis par le miroir et tombaient sur la partie sensible d'une cellule photoélectrique au potassium. Le courant photoélectrique, amplifié par des lampes à 3 électrodes, était susceptible d'actionner des appareils enregistreurs.

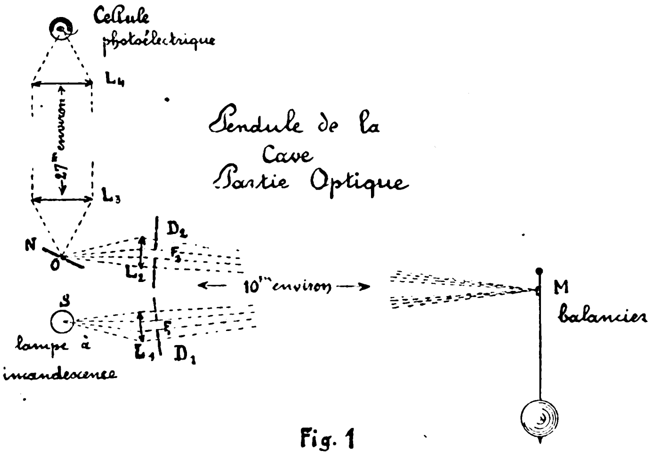

Fig. 1

Dans la réalité, le problème présentait des difficultés pratiques, car les pendules de l'Observatoire de Paris sont placées dans des caves à une profondeur d'environ 27 mètres. La figure 1 montre la solution adoptée.

Sur le balancier de la pendule était placé un miroir plan M. La lentille L_1 donnait sur ce miroir l'image d'une puissante lampe électrique S.

Dans un diaphragme D_1, placé devant la lentille, était ménagée une mince fente horizontale F_1. Ce sont les rayons émanés de cette fente qui venaient se réfléchir en M.

Le système était disposé de telle façon que lorsque le balancier était vertical, les rayons réfléchis par M pouvaient passer à travers une autre fente horizontale ménagée dans le diaphragme D_2. Ils étaient reçus par une lentille L_2 et donnaient au point O sur le miroir N une image de la lampe. Les rayons réfléchis par le miroir N, transformés en un faisceau parallèle par la lentille L_3, s'élevaient dans un puits vertical de 27 mètres de hauteur et étaient concentrés par la lentille L_4 sur la cellule photoélectrique.

On voit qu'à chaque passage du balancier par la verticale, la cellule était illuminée pendant un temps très court.

Le courant photoélectrique était amplifié par un amplificateur à deux étages, le premier comportant une lampe bigrille et le second un système de trois lampes ordinaires en parallèle (fig. 2).

Ce courant amplifié, de l'ordre de 4 à 5 milliampères, était susceptible d'actionner la plume d'un enregistreur, ou bien était envoyé dans le primaire du transformateur d'entrée d'un amplificateur à basse fréquence et donnait des « tops » perceptibles au téléphone.

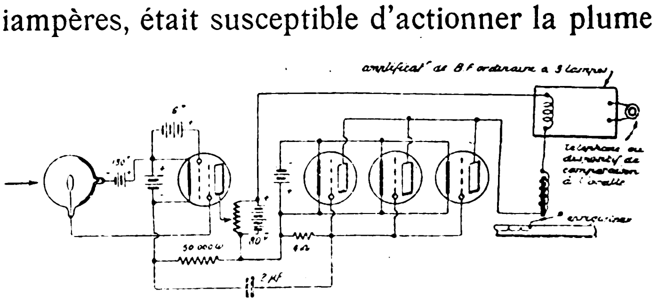
amplificatʳ de B.F ordinaire à 3 lampes
6ᵛ
130ᵛ
80ᵛ
50 000 ω
4 Ω
2 μF
téléphone ou dispositif de comparaison à l'oreille
enregistreur

FIG. 2. — Amplification pour la pendule de la cave de l'Observatoire de Paris.

Comme appareil enregistreur, on utilisait un des appareils à noir de fumée utilisés pour le repérage du son pendant la guerre; le système déviant étant un oscillographe à style Abraham du type dit électrométrique. La vitesse de déroulement du papier était de 6 centimètres par seconde.

Sur la même bande étaient inscrites les vibrations d'un diapason à 50 périodes entretenu électriquement sans contact par des lampes à 3 électrodes suivant le procédé indiqué simultanément en France par Henri ABRAHAM, en Angleterre par ECCLES et JORDAN.

Les bandes ainsi obtenues ont été examinées au moyen d'une machine à diviser et ont encore mis en évidence une inégalité des diverses secondes (on ne comparait bien entendu que les secondes de même signe). Ces irrégularités étaient moindres que celles relevées par la fermeture d'un circuit électrique par le balancier, mais elles étaient encore importantes (de l'ordre de 5 à 6 millièmes) et présentaient nettement une périodicité de l'ordre de la minute; ce qui indique bien que le mécanisme de la pendule devrait en être cause. Signalons que M. P. P. QUAYLE, du « Bureau of Standards », a également reconnu par la suite que les meilleurs chronomètres de cet établissement présentaient également, pour la durée de leurs diverses secondes, des irrégularités de plusieurs millièmes (1).

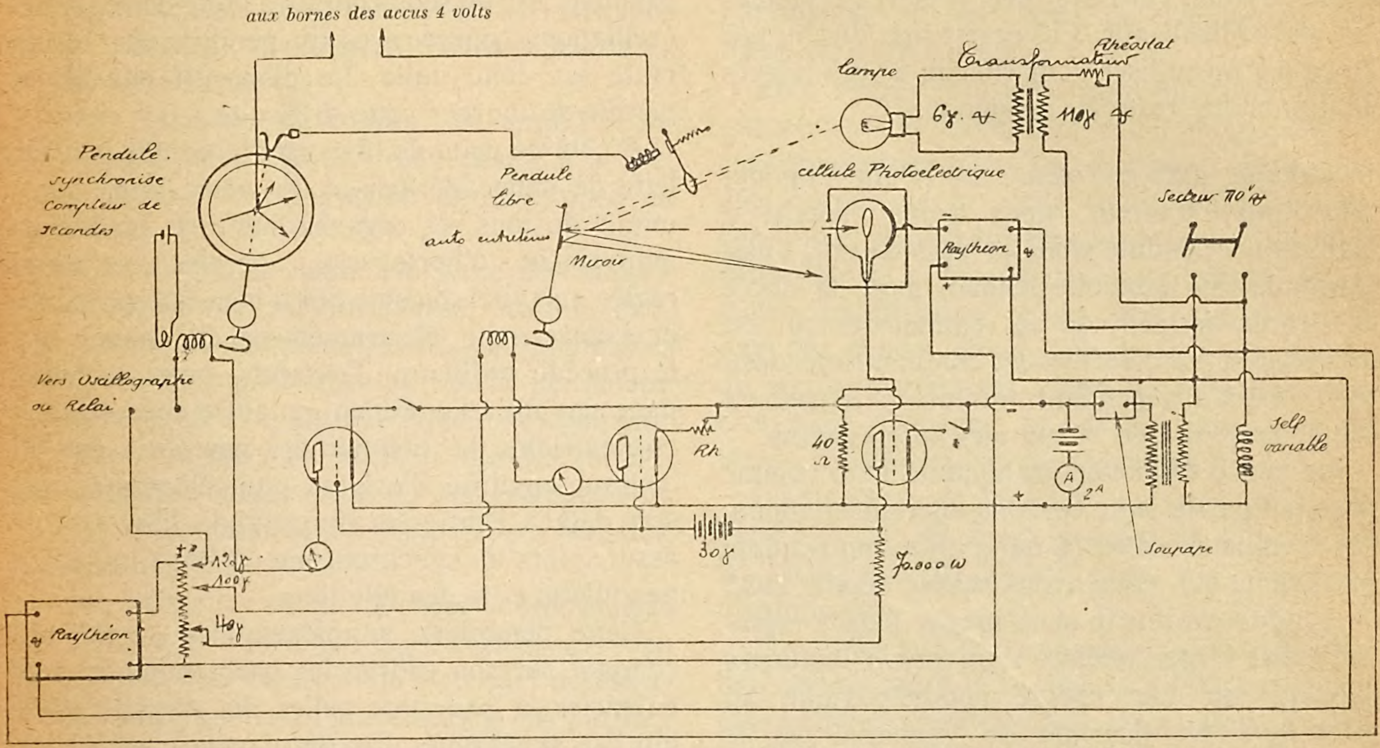
aux bornes des accus 4 volts
Pendule. synchronise Compteur de secondes
Pendule libre
auto entretenu
Miroir
Vers Oscillographe ou Relai
Rh
30 v
12 v
100 v
40 v
Raytheon
Lampe
Transformateur
Rhéostat
6 v. ~
110 v ~
cellule Photoelectrique
Raytheon
Secteur 110 v ~
40 Ω
70.000 ω
A
2 A
Self variable
Soupape

FIG. 4. — Pendule photoélectrique auto entretenu. (Installation faite à l'Observatoire de Paris.)

C'est évidemment dans le mécanisme de la pendule qu'il faut chercher la cause de ces irrégularités.

On sait que tout choc, qui n'est pas donné au balancier à l'instant précis où il passe par sa position d'équilibre, agit sur la période. C'est ce qui se produit avec les dents des rouages de la pendule qui, n'étant pas exactement taillées, n'agissent pas toutes au moment précis où le balancier passe par sa position d'équilibre. Il en résulte donc une variation pour la période du balancier.

Les considérations qui précèdent montrent que le balancier de la pendule est tantôt légèrement en avance, tantôt légèrement en retard sur le balancier idéal qui serait réalisé s'il n'existait aucune cause de perturbation et dont la période serait parfaitement régulière, la somme algébrique des écarts étant nulle au bout d'un temps de l'ordre de la minute.

Si donc on cherche à déterminer l'instant d'un signal ou d'un « top » enregistré sur la même bande que les battements de la pendule, en mesurant sur la bande l'intervalle de temps qui sépare ce signal ou « top » de la seconde la plus voisine, on commet une erreur égale à la différence de phase entre le balancier vrai et le balancier idéal au moment de la seconde servant de repère. Si, au contraire, on détermine le même instant en mesurant sur la bande les intervalles de temps qui séparent le signal ou « top » des 60 secondes les plus voisines, et en faisant la moyenne, l'erreur est éliminée.

Cette manière de procéder est longue, et de plus elle est difficile à appliquer quand il s'agit de comparer, non pas un signal ou « top » aux battements d'une pendule, mais une série de signaux radios dont l'enregistrement est parfois incomplet, ou une série de battements d'une autre pendule pour déterminer la différence de période des deux pendules et leur état relatif. La précision des mesures laisse donc à désirer en général.

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(1) *Journal of the Franklin Institute*, mars 1927, p. 407.

Les erreurs ainsi occasionnées se manifestent par exemple dans les publications du Bureau international de l'Heure relatives à l'opération mondiale de longitudes faite en octobre-novembre 1926. On note, en particulier, des écarts assez importants, pour les heures de réception des signaux radiotélégraphiques d'ANNAPOLIS, entre les divers relevés effectués sur une même bande. Ces écarts sont dûs au fait qu'on n'a pu utiliser pour chacun de ces relevés qu'un nombre restreint de signaux.

Pendule auto-entretenu. — Pour éliminer cette cause d'erreur, nous avons cherché à réaliser un pendule dont la période soit aussi constante que possible pendant toute la durée de l'enregistrement de ses battements qui est nécessaire pour effectuer les comparaisons désirées, c'est-à-dire pendant un temps relativement court, de l'ordre de 15 minutes au maximum.

Au lieu d'employer un pendule libre comme l'ont fait par la suite certains expérimentateurs, par exemple M. BROWN qui a utilisé un pendule géodésique (1), nous avons réalisé depuis 1925 un pendule entretenu sans aucune liaison mécanique par l'intermédiaire d'un rayon lumineux agissant sur une cellule photoélectrique au potassium. Le principe de l'appareil est le suivant :

Un balancier, du type de celui des pendulettes entretenues électriquement suivant le principe indiqué il y a longtemps par Cornu, porte un aimant plongeant horizontalement dans une bobine. Pour entretenir son mouvement, il suffit d'envoyer, chaque fois que le balancier passe par la position d'équilibre, un courant de sens convenable dans la bobine. Pour obtenir ce courant sans que le balancier ait à fermer aucun circuit par un contact matériel pouvant troubler sa période, sur la tige du balancier a été fixé un miroir qui est frappé par un rayon lumineux émané d'une lampe et, venant après réflexion, éclairer la cellule à l'instant convenable. Le courant qui est alors engendré dans celle-ci est envoyé dans un amplificateur à lampes puis dans la bobine. La durée de l'impulsion n'est que de 5 à 6 centièmes de seconde et se produit exactement à l'instant où le balancier passe par la verticale.

Une difficulté se présentait, le pendule (qui bat la demi-seconde) éclaire la cellule chaque fois qu'il passe par la position d'équilibre et par suite, le courant photoélectrique étant toujours de même sens, l'action sur l'aimant du balancier est de sens contraire pour deux demi-oscillations consécutives du pendule. La résultante est donc nulle. Le dispositif suivant a permis de tourner cette difficulté :

A côté du pendule libre est placée une pendulette de même période et de même type, mais munie de tous ses organes habituels (contacts, mouvement d'horlogerie, aiguilles, cadran), réglée au synchronisme aussi bien que possible, et s'entretenant électriquement elle-même par le procédé ordinaire. Toutefois, outre le bobinage servant à l'entretien, un autre enroulement concentrique au premier est parcouru par le courant amplifié d'origine photoélectrique qui sert déjà à l'entretien du pendule libre et qui assure alors un synchronisme parfait entre cette pendulette et le pendule libre.

Cette pendulette supplémentaire permet de compter sur son cadran les oscillations de son balancier et par suite celles du pendule libre qui est synchrone. En outre cette pendulette commande un dispositif qui, toutes les demi-secondes de même polarité (paire ou impaire), ferme un circuit commandant un obturateur mû électriquement et disposé sur le trajet du rayon réfléchi (1). Dans ces conditions, la cellule ne peut recevoir la lumière que toutes les deux demi-secondes et le pendule libre entretient automatiquement son mouvement sans difficultés.

Le dispositif complet est le suivant (fig. 3 et 4) :

Une lampe à filament rectiligne envoie sa lumière sur un miroir sphérique donnant de ce filament une image sur un diaphragme muni d'une fente placée devant la cellule. L'appareil est réglé de telle façon que l'image se forme exactement sur la fente quand le balancier est vertical. On obtient de cette façon des départs très brusques du courant photoélectrique.

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(1) *Bulletin of the National Research Council*, t. II, partie 12, n° 56, novembre 1926.

(1) En réalité, pour obtenir un bon fonctionnement de l'obturateur, on établit un léger décalage entre le pendule libre et la pendulette-compteur. On arrive à ce résultat en réglant la pendulette à une période très légèrement différente de celle du pendule auto-entretenu. Dans ces conditions on sait qu'une légère différence de phase s'établit entre le balancier synchronisateur et le balancier synchronisé.

En série avec l'anode de la cellule photoélectrique est placée une résistance de 40 mégohms connectée d'autre part entre le filament et la grille d'une lampe à 3 électrodes. Toute production de courant par la cellule provoque une chute de tension aux bornes de la résistance et par suite une variation du courant plaque de la première lampe (1).

Cette variation de courant est amplifiée au moyen d'un amplificateur du type dit « à courant continu », comportant deux lampes en parallèle.

Dans le circuit plaque de l'une des lampes est placée la bobine d'entretien du pendule libre, dans le circuit plaque de l'autre sont intercalées les spires de synchronisation de la pendulette qui commande l'obturateur et sert de compteur de temps. On place en outre en série les bobines de l'appareil enregistreur ou les enroulements d'un relais commandant les appareils d'émission du poste de T. S. F.

Pour éviter les sujétions et les ennuis causés par l'usage et l'entretien des batteries de piles nécessaires à la création du potentiel accélérateur de la cellule et à l'alimentation des plaques des lampes triodes, on utilise le secteur éclairage à courant alternatif qui alimente l'Observatoire de Paris. La tension du secteur, élevée à une valeur convenable par un transformateur, est redressée au moyen d'un « raythéon », les ondulations du courant étant adoucies par un filtre. Un raythéon alimente la cellule, un autre les plaques des lampes.

Les filaments des lampes sont également alimentés par le secteur alternatif, dont le courant est redressé par une soupape au tantale, une batterie d'accumulateurs de 4 volts étant placée comme batterie tampon. Il ne reste comme batterie de piles que la batterie de liaison entre les deux étages de lampes, batterie qui ne débite pas et ne nécessite aucun entretien.

La lampe qui éclaire la cellule est également alimentée par le secteur alternatif.

La tension du secteur étant variable, l'intensité lumineuse de la lampe, ainsi que le potentiel accélérateur de la cellule, sont suceptibles de varier. Il en résulte une variation possible de l'impulsion et par suite de l'amplitude du balancier auto-entretenu.

Pour remédier à cet inconvénient, la lampe qui alimente la bobine d'entretien de ce pendule est très peu chauffée. Les variations de tension aux bornes de la grille conduisent dans ces conditions le courant plaque dans la région de saturation. Le chauffage du filament de la lampe amplificatrice a été choisi de telle sorte que des chutes même importantes de tension du secteur permettent encore au courant photoélectrique d'amener le courant plaque de la lampe dans la région de saturation (1).

L'amplitude du balancier est très faible, de l'ordre de 3°, de façon à réduire au minimum les variations de période dues aux variations d'amplitude.

Pour exécuter des mesures avec cet ensemble, nous avons employé le dispositif déjà mentionné plus haut et comportant un enregistrement avec oscillographe ABRAHAM sur un appareil à noir de fumée, un diapason étant enregistré en même temps.

Voici les résultats de quelques-unes de ces mesures :

L'ensemble ayant été mis en marche à 8 heures du matin, le premier enregistrement a été effectué à 9 heures du matin, le second à 16 heures.

10 tops consécutifs ont été relevés chaque fois.

Enregistrement de 9 heures. — Durée *moyenne* de la période du pendule en fonction de la période du diapason :

49,972 périodes par seconde.

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(1) Pour amplifier le courant photoélectrique qui est très faible, il convient en général d'utiliser une lampe bigrille qui fonctionne sous faible tension plaque. Ceci évite l'ionisation par choc des gaz restants et, par suite, la tendance à la décharge de la grille par les ions positifs. Dans le cas actuel, avec le fort éclairement réalisé, une lampe ordinaire à trois électrodes peut convenir et a été employée parce qu'elle présente l'avantage d'éviter la batterie de piles qui serait nécessaire pour polariser la grille intérieure d'une lampe bigrille.

(1) D'autre part la tension des grilles des lampes de l'amplificateur à courant continu est réglée de façon que leur courant plaque soit nul lorsque la cellule n'est pas éclairée.

Numéro des secondes.	Écart relatif de la période mesurée par rapport à la période moyenne exprimée en dix-millièmes de seconde.
1	−4
2	+8
3	−8
4	−3
5	+4
6	−1
7	+4
8	+1
9	−1
10	0

Enregistrement de 16 heures. — Durée *moyenne* de la période en fonction de la période du diapason :

49,98 périodes (la température avait légèrement varié).

Numéro dos secondes.	Écart en dix-millièmes de seconde.
1	−2
2	+2
3	−6
4	+6
5	+8
6	−5
7	−3
8	−1
9	0
10	+1

Ces résultats montrent bien qu'on peut considérer le pendule auto-entretenu comme donnant des oscillations constantes de période constante à moins de 1 millième de seconde près. La précision est même peut-être plus grande, car avec le procédé d'enregistrement au noir de fumée, le millième est tout ce qu'on peut garantir, en raison de l'épaisseur du trait, du frottement du style, etc., malgré toutes les précautions prises dans le relevé des bandes, et il convient de ne *pas perdre de vue que les nombres exprimés en dix-millièmes que nous avons donnés ne sont là que parce qu'ils résultent des mesures effectuées mais n'ont en réalité aucune valeur réelle.*

La marche du pendule s'est montrée très régulière. On a enregistré simultanément les battements de la pendule directrice de l'Observatoire et du pendule auto-entretenu pendant deux minutes au commencement et à la fin d'un même quart d'heure. On pouvait ainsi déduire la durée de la période du pendule auto-entretenu en fonction de la période de la directrice en faisant porter les mesures sur 900 secondes.

Une première mesure donnait pour le pendule auto-entretenu une période trop courte de 7/100.000ᵉ. Une deuxième mesure effectuée 3 heures après donnait la valeur de 8/100.000ᵉ.

Toutefois il importe de remarquer que ces mesures ont été effectuées à des instants de la journée où les variations du secteur électrique d'alimentation étaient relativement faibles, ce qui n'a pas lieu la nuit et à la tombée du jour et à ce moment malgré les précautions prises on ne peut répondre d'obtenir la même régularité. Il ne semble donc pas qu'on puisse *à l'heure actuelle au moyen de l'appareil tel qu'il a été utilisé* avec l'alimentation avec le courant du secteur d'éclairage, constituer un garde-temps conservant avec certitude le 1/100ᵉ de seconde après vingt-quatre heures de fonctionnement. Toutefois avec une alimentation électrique n'ayant que de faibles variations, ce résultat pourrait être largement atteint et même dépassé.

Il conviendra toujours en tous cas, lorsqu'on aura utilisé le pendule auto-entretenu, de le comparer aussi rapidement que possible à la pendule directrice pour déterminer sa période.

Le pendule auto-entretenu, dont la période a été très exactement déterminée, se prête particulièrement bien à la détermination de la fréquence des diapasons qui sont ensuite employés pour la mesure des fréquences, en faisant usage du multivibrateur Abraham ou d'autres dispositifs.

Emissions de signaux radiotélégraphiques horaires. — Pour comparer avec précision les signaux radiotélégraphiques horaires rythmés, appelés aussi signaux scientifiques, aux battements de la pendule astronomique locale, il est évidemment nécessaire que ces signaux soient émis avec une parfaite régularité, c'est-à-dire soient également espacés. Il n'en est malheureusement pas ainsi pour le moment car les pendules qui commandent ces signaux ont les irrégularités dues aux rouages et qui ont été signalées et discutées plus haut.

L'emploi du pendule auto-entretenu a permis, au cours d'essais faits à l'Observatoire de Paris, de faire disparaître ce défaut et d'obtenir une émission de signaux dont l'espacement est constant à moins d'un millième près. Il suffit pour cela d'intercaler dans le circuit plaque de la deuxième lampe du dispositif de la figure 4 les enroulements des relais BAUDOT qui, de l'Observatoire de Paris, commandent les postes radio-télégraphiques de la Tour EIFFEL, d'ISSY et de BORDEAUX LAFAYETTE, pour faire émettre par ceux-ci des signaux commandés par le pendule auto-entretenu. Toutefois certaines difficultés nouvelles se présentent alors : Tout d'abord le pendule auto-entretenu doit donner 61 battements par minute. De ce côté, il n'y a pas de difficultés, c'est une question de réglage du balancier.

Le premier des signaux doit en outre être émis, aussi exactement que possible, à une minute ronde. Il faut donc que le pendule auto-entretenu donnant 61 battements par seconde, soit maintenu exactement à l'heure de la pendule garde-temps moyen de l'Observatoire, ou plutôt que chaque groupe de 61 battements du pendule auto-entretenu commence à l'instant de la première seconde de chacune des minutes du garde-temps, pendant toute l'émission des signaux.

Voici comment on y arrive. Toute la journée, sauf au moment des battements, le pendule auto-entretenu est synchronisé par la pendule garde-temps moyen, en substituant à son courant d'entretien photoélectrique, le courant fourni par le garde-temps moyen. Toutefois ce courant ne doit être envoyé dans la bobine du pendule auto-entretenu qu'une fois par minute, à l'instant de la première seconde de chaque minute du garde-temps, puisque celui-ci donne 60 battements par minute de temps moyen tandis que le pendule auto-entretenu donne 61 battements pendant le même intervalle de temps.

Cinq minutes avant l'émission des signaux horaires, on supprime la synchronisation du pendule auto-entretenu par la pendule garde-temps moyen, et l'on met en route le dispositif d'entretien photoélectrique. Puis dans la seconde qui précède la minute, on ferme le circuit qui fait agir le courant photoélectrique sur les relais BAUDOT commandant les émissions radiotélégraphiques.

Dans ces conditions, on a pu constater que le premier signal horaire était émis moins de 1/10 de seconde après la minute ronde de temps moyen, ce qui est suffisant pour les besoins de la pratique. L'heure exacte de ce premier signal est d'ailleurs donnée par la suite par le *Bulletin du Bureau International de l'Heure* pour les observateurs qui ont besoin d'une plus grande précision.

On peut se demander si les divers relais intercalés entre l'Observatoire de Paris et les postes d'émission, n'occasionnant pas des irrégularités dans les émissions, par suite du retard qu'ils apportent, retard qui pourrait être variable. Pour le vérifier, nous avons enregistré pendant cinq minutes à l'Observatoire de Paris sur la même bande à la fois les courants photoélectriques du pendule auto-entretenu et les signaux radiotélégraphiques, et nous avons mesuré le retard entre les deux enregistrements, au début et à la fin de la série de signaux.

Les résultats obtenus sont les suivants :

Enregistrement du début :

Numéro du top.	Différence entre le retard moyen et le retard individuel en dix-millièmes de seconde.
1	+ 3
2	— 4
3	+ 4
4	— 4
5	+ 1

Enregistrement de la fin :

1	+ 6
2	+ 1
3	— 6
4	— 4
5	+ 3

D'autre part, les deux valeurs moyennes ne différaient que de 1/10.000 c'est-à-dire d'une quantité inférieure à la précision des mesures.

Ces résultats montrent donc que le dispositif envisagé permettra d'émettre des signaux dont l'espacement sera constant à moins d'un millième de seconde près. Ce dispositif sera mis prochainement en service à l'Observatoire de Paris.

Détermination de l'heure. — Nous ne mentionnerons que pour mémoire une autre application des cellules photoélectriques à l'astronomie de position, et par suite à la mesure du temps : la détermination des instants du passage des étoiles au méridien. Cette utilisation a fait l'objet d'une publication dans les *Proceedings of the Institution of Electrical Engineers*, t. XIII, n° 4, août 1925, p. 461.

Etude de la gravité. — Signalons également qu'il est aisé de créer un dispositif analogue à celui de la pendule de la cave de l'Observatoire de Paris (fig. 1 et 2), mais beaucoup simplifié, pour mesurer la période d'un pendule libre établi pour déterminer l'intensité de la pesanteur (fig. 5).

Nous devons mentionner que divers expérimentateurs ont également essayé d'utiliser des cellules photoélectriques combinées aux lampes à plusieurs électrodes pour des buts identiques à ceux que nous poursuivons depuis 1922 et que nous avons atteints depuis 1925.

Quayle, au « Bureau of Standards » pour l'étude des diapasons, a utilisé comme indicateur de temps un pendule libre portant un miroir réfléchissant un rayon lumineux venant actionner une cellule photoélectrique (1).

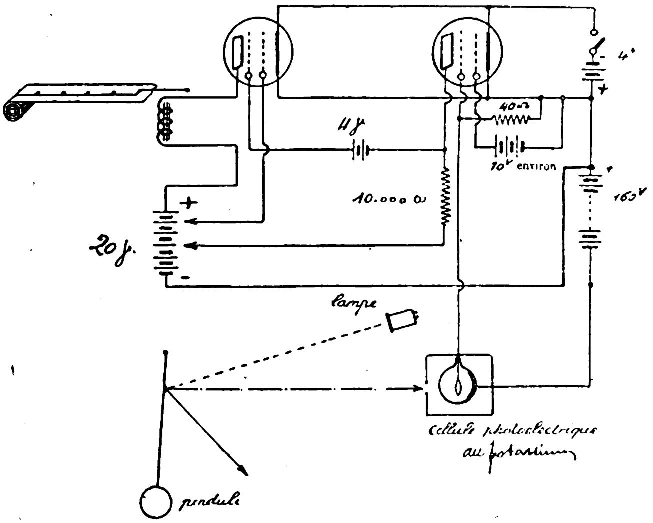

Fig. 5. — Dispositif d'enregistrement photo-électrique pour pendule géodésique.

De même Gould, au « Bureau of Standards », aurait cherché récemment à commander l'émission de signaux horaires par un pendule libre actionnant une cellule sensible à la lumière.

Dans les laboratoires de la Bell C^ie, il aurait également été réalisé en 1926 un pendule auto-entretenu par l'action d'un rayon lumineux. Le mode opératoire était un peu différent de celui que nous avions indiqué. Le courant photoélectrique provoquait la fermeture d'un relais et celui-ci fermait sur la bobine excitatrice de la pendule le circuit d'une pile.

L'action photoélectrique remplaçait donc uniquement l'ensemble cliquet roue dentée utilisé habituellement.

Precision Determination of Frequency,

by J. W. Horton and W. A. Marrison,
Bell Telephone Laboratories, New York (N. Y., U. S. A.)

When we speak of the frequency of a wave or, in fact, when we use the word frequency in any connection, we mean, in general, the number of times a periodic occurence takes place within a given interval of time. Consequently the frequency of a recurrent phenomenon is the reciprocal of the time interval required for the variation to pass through one cycle provided the duration of any given cycle is identical with that of any other, or, in other words, that the variation has a constant frequency. Since, therefore, frequency may be expressed completely in terms of time, it is neither necessary nor desirable to have any fundamental standard of frequency other than the accepted standard time interval.

The present standard of time is, of course, the sidereal day, which is the time required for the earth to make one complete revolution in space. A succession of sidereal days, therefore, constitutes a recurrent phenomenon the duration of any cycle of which is, by definition, equal in time to the duration of any other cycle. In other words, the variation has the essential characteristics required of a standard of frequency. The mean solar day, in terms of which all measurements of time are expressed, must be considered as a practical standard the determination of which is made in terms of the sidereal day.

There is a unique difference between a standard of time — or frequency — and standards of most other quantities. For example, the standard unit of mass or the standard unit of length may be given physical embodiments and may be put in a safe where their identity will be preserved over extended periods. Unlike a mass or a length, an interval of time cannot of itself be preserved nor can it readily be given a physical embodiment which by virtue of its great constancy may be used as a fundamental standard. At present, therefore, all precision measurements of time must be referred directly to the rate of the earth's rotation.

Having once concluded that the fundamental

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(1) *Journal of the Franklin Institute,* mars 1927, p. 407.

standard of time is, *ex officio*, the fundamental standard of frequency, there remains simply the problem of comparing the rate of any periodic variation which is to be measured with the rate of the earth's rotation.

If there were available only the periodic phenomenon of unknown rate and the intervals defined by the earth's rotation, it would be necessary that the unknown phenomenon maintain its rate unchanged throughout an entire interval covered by one rotation of the earth. In this case the measurement would consist in counting the number of times the unknown variation repeated itself during the interval. Due, however, to the availability of means for accurately defining time intervals less than the sidereal day to the required precision, it is, of course, unnecessary to maintain the rate of the variation to be measured over the longer interval. When we recognize that any means used for evaluating the time of short intervals is itself executing periodic variations of equal duration, we realize that it is a secondary standard of frequency as well as a secondary standard of time. Thus the Riefler clocks at the National Bureau of Standards and at the United States Naval Observatory become our most practical working standards of frequency (1). In terms of the oscillations of their pendulums their frequency may be said to be unity when referred to the second.

A secondary standard such as a high grade seconds pendulum is unsuited to the actual determination of the frequency of such variations as are of interest in electrical communication, both because of the excessively high ratio between the rates of the occurrences to be compared and because, in its usual form, the motion of a clock pendulum is not well suited for comparison with electrical variations. It is desirable, therefore, as the first step in the precision measurement of the frequencies of electric waves, to provide a suitable reference standard. The requirements of such a standard are that its rate should be of the same order of magnitude as the rates to be measured, that the form of the variation which it controls should be suitable for convenient comparison with the variations to be measured, and, finally, that its rate shall be sufficiently constant so that the total number of variations which it executes in a time of such duration that it may be defined with high precision may be taken as the rate of the variation over shorter intervals of time. This last requirement again emphasizes the inherent similarity between secondary standards of frequency and secondary standards of time. For convenience, it should also be possible to adjust the rate of the frequency standard to some prescribed value within such limits that the error may be neglected in the majority of measurements.

Original equipment. — A secondary standard having the characteristics mentioned above was described several years ago by the authors of the present paper in collaboration with Mr. N. H. Ricker. This standard consists of a 100 cycle tuning fork maintained in vibration by an amplifier regeneratively connected through the fork by electromagnetic coupling. Means are provided for obtaining from the electrical portion of the resulting oscillating system a sinusoidal alternating current the frequency of which is constant to high precision. Standards of this general type are becoming extensively used, as will be seen by referring to the bibliography. A brief survey of the present status of this 100 cycle standard is of interest not only because of the improvement in its performance since the original report was presented, but also because the experience which has been gained from its use has been of value in the development of standards of still greater utility.

The fork which was described in the paper already referred to ran continuously from April, 1923 to May, 1927 except for four intervals totalling about three days. Of these interruptions two were from accidental causes and two for the purpose of making minor changes in the system. Throughout this entire period the temperature of the fork, as maintained by its thermostatically controlled water bath, was held at approximately the value for which its rate is 100 cycles. Since the temperature coefficient of frequency for the particular fork in question is 0.0109 per cent per degree Centigrade, it is necessary to keep the temperature to within 0.01° C of the prescribed value in order that the rate shall be correct to one part in a million. It has been found that the temperature coefficient, instead of being a detriment, has furnished an excellent means for adjusting the frequency, making possible small changes the amounts of which may be accurately predetermined. During the four years of operation, the change in the frequency of the entire system due to aging of the fork and to all other causes, as determined by the change in the temperature — indicated by a Beekman thermometer — at which the rate is exactly 100 cycles, has been less than 0.004 per cent.

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(1) This, of course, applies to the United States only; but a similar statement may be made of any national reference standard of time.

The synchronous motor described in the previous paper has been replaced by an improved form shown in Fig. 1. The original motor was coupled to the clock by means of a gear reduction driving a commutator, from which seconds impulses were supplied to a clock train having an electromagnetic stepping device. The present motor is geared directly to the clock train, thus completely avoiding errors due to the mechanism. In spite of the lack of confidence expressed by many people in the impulse type of synchronous motor, both of these mentioned have been found to give entirely satisfactory operation over long periods of time. In fact, the only occasions on which the motors have ever stopped in service have been those on which power was taken off some portion of the system.

During tests on this frequency standard, it was found that it constituted a far more reliable timekeeper than the electrically maintained pendulum clock which was used to obtain the data already published. The pendulum clock was, therefore, dispensed with and all measurements of the rate of the fork are now made by direct comparison with the mean solar day as defined by the radio time signals sent out from the United States Naval Observatory. By means of its second hand, which is concentric with the hour and minute hands and which rotates uniformly, the daily error in the fork controlled clock may be determined to 0.2 second, giving its average rate for the day to approximately three parts in one million. For greater accuracy there is provided, in the gear train between the motor and the clock, a shaft rotating once per second and operating a contact by means of a cam. Pulses from this contact may be recorded simultaneously with the radio time pulses on a moving paper tape by means of a two-element siphon recorder. From the record thus obtained, the error of the clock may be determined to 0.01 second, increasing the accuracy of the determination about 20 times.

The data shown by the chart of Fig. 2 are typical of the performance of this system. On this chart the error in the clock reading is plotted against days. The rate is obtained from the slope of the curve; when it is straight the rate is constant; when it is horizontal the rate is exactly 100 cycles. Attention is called not only to the constancy of the rate, which at no time during the period covered by this record departed more than 0.0007 per cent from the mean rate, but also to the precision with which the system was adjusted to the desired absolute value.

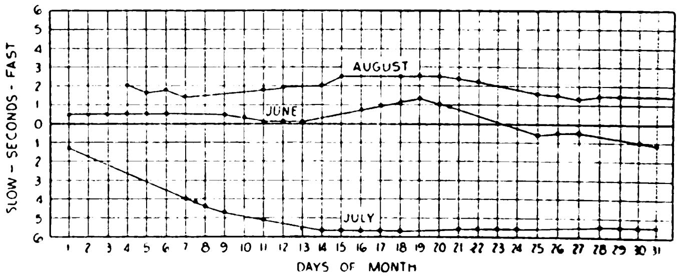

Fig. 2. — Daily Error in Clock Controlled by 100 Cycle Frequency Standard.

Mean Rate for Three Month Period : 99.99991 Cycles.
Average Deviation from Mean Rate : 0.00016 Cycles.
Maximum Deviation from Mean Rate : 0.00067 Cycles.

A method for the comparison of high frequencies with the 100 cycle standard is also outlined in the previous paper. This method involves the use of harmonic producers and of harmonic selecting networks. By repeated harmonic production and selection it is possible to obtain any multiple of the fundamental frequency to a limit determined by the care taken. Theoretically the process may be extended indefinitely, but practically there are difficulties in going beyond two or three stages. These difficulties arise from small irregularities in the frequency or in the amplitude of the current in the initial stages of the system, all of which result in frequency irregularities in the final wave. While these may be entirely negligible in the wave in which they originate, they are enormously magnified in subsequent stages. With ordinary care it is feasible to obtain from the 100 cycle standard current a current having a frequency of 100,000 cycles per second or even higher. By the use of a cathode ray oscillograph, an independent oscillator may be adjusted so that its frequency is an exact multiple of the frequency of the current thus obtained. This process may be repeated by successive stages up to the limit of the oscillograph tube.

The system, therefore, contains three essential elements : The regeneratively driven fork constitutes a source of alternating current, the frequency of which is 100 cycles to a very high precision. The synchronous motor and the gear train of the clock constitute a rate reducing mechanism having a ratio of approximately ten million to one. The harmonic producer system and the cathode ray oscillographs may be operated so as to increase the frequency in the ratio of several thousand to one. It is, therefore, practicable by means of this system to determine the frequency of an electric wave, using the mean solar day as a standard, when the ratio between them does not exceed about 10^{11}.

Present equipment. — At the time the development of the 100 cycle standard was undertaken, the frequency range over which measurements were required to high precision did not exceed several hundred thousand cycles per second. For this work the frequency chosen for the secondary standard was quite satisfactory. Since then, however, more and more attention has been given to the higher frequencies until it has become necessary to make measurements to a very high degree of accuracy up to several million cycles per second. Any appreciable increase in the frequency of the reference standard is obtained at the expense of an increase in the ratio between its rate and the mean solar day. This is not particularly objectionable, however, since the comparison between these two rates may be made with permanently adjusted apparatus designed expressly for this fixed ratio, whereas comparisons between unknown rates and the reference standard must be made by carefully adjusting apparatus capable of working over a wide range of ratios. More important still, it has been found that the frequency of a given current can be controlled by a current of higher frequency with much greater stability than by a current of lower frequency. Because of these various factors, therefore, there is need for a standard of the general type just described but having a fundamental frequency many times higher. Such a standard has been developed and some preliminary data as to its constancy of rate have been obtained.

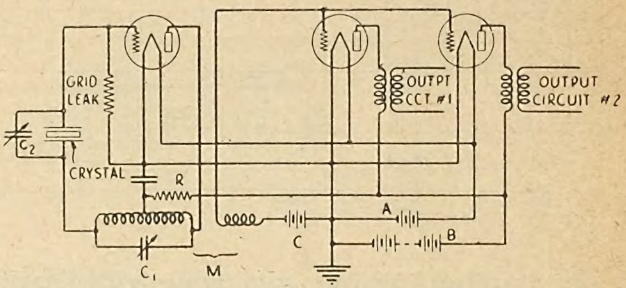

Fig. 3. — Circuit of Oscillator for High Frequency Standard.

The high frequency generator of this standard is a 50,000 cycle quartz crystal controlled oscillator having circuit elements and controls that give a high degree of stability. The circuit of this oscillator is shown in Fig. 3. It is of the Hartley type, differing from the usual form only in having a resonant piezo-electric crystal connected in the grid circuit. This type of circuit is used because it is convenient, by means of circuit adjustments, so to control the phase of the regenerative feedback that maximum stability is obtained. Being an efficient oscillating circuit it also permits of using the crystal loosely coupled to the electrical circuit, thus permitting full advantage to be taken of the very low decrement of the crystal.

The crystal is adjusted by lapping so that the frequency of the circuit controlled by it is 50,000 cycles exactly at a given operating temperature. Minute corrections that are subsequently required can be made by means of a small variable condenser in parallel with the crystal.

The crystal controlled oscillator is coupled to two output circuits through vacuum tubes, having their grids in parallel, loosely coupled to the tuning coil. In this way considerable output current at 50,000 cycles can be obtained with no possibility of reaction on the oscillator from external circuits.

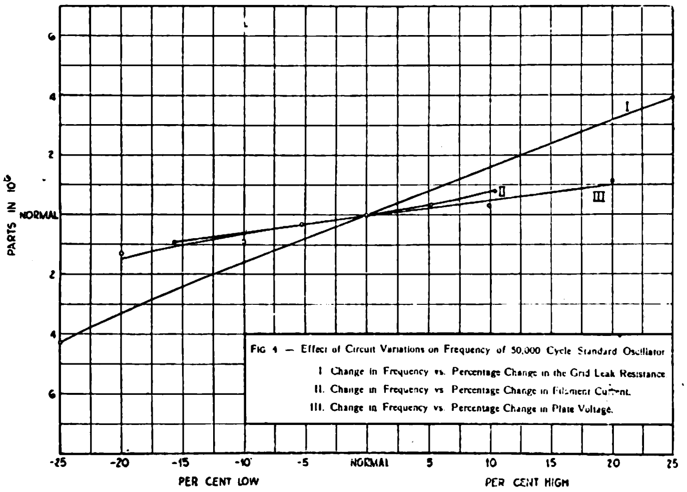

FIG 4 — Effect of Circuit Variations on Frequency of 50,000 Cycle Standard Oscillator

I. Change in Frequency vs. Percentage Change in the Grid Leak Resistance.
II. Change in Frequency vs. Percentage Change in Filament Current.
III. Change in Frequency vs. Percentage Change in Plate Voltage.

The three curves in Fig. 4 show the frequency stability of the oscillator under variations in filament current, plate voltage, and grid leak resistance. The departure from normal frequency in parts in a million is plotted against departure in per cent from normal filament current, plate voltage, and resistance.

The variation with grid leak resistance is greatest, but due to the use of a special type of resistance that is expected not to vary as much as 0.1 per cent in service, the variations in frequency due to this source are negligible.

The variation with battery voltage is the most serious factor, being in the same sense for both batteries. These voltages are controlled at present to about ± 2 per cent, therefore variations in frequency of about three parts in ten million may be expected due to them. Of course, as has been done in other cases, it is possible to compensate the circuit for variations in voltage so that any tendency for a change in frequency in one direction is offset by an equal and opposite tendency.

The curves of Fig. 5 show the variations in frequency of the oscillator for changes in the condensers C_1 and C_2. These curves show variations in frequency in parts in a million and are plotted, in the case of condenser C_2, against dial settings, and in the case of condenser C_1, against the capacity in micro-microfarads. Due to the linearity of the adjustment obtained by condenser C_2 it is possible to make predetermined minute changes in the operating frequency. The proper adjustment for condenser C_2 is very nearly that value for which the change in frequency with capacity is zero. Under this condition the small changes in the electrical circuit due to temperature variations have an entirely negligible effect on the frequency.

The quartz crystal in its mounting is shown in Fig. 6. It vibrates in the direction of its length and is supported on a short piece of felt at the center in order to allow free vibration. It is held in place and separated from the plates by silk threads. The logarithmic decrement of this crystal in its mounting, the whole being measured as an electric circuit, is about 0.00012.

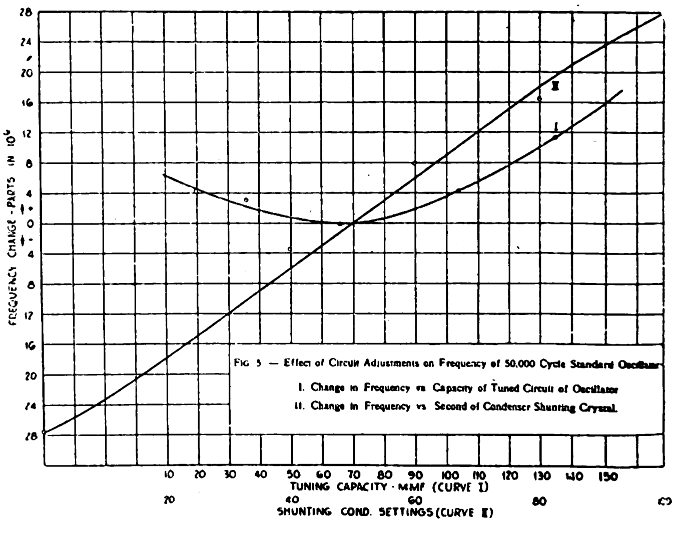

FIG 5 — Effect of Circuit Adjustments on Frequency of 50,000 Cycle Standard Oscillator

I. Change in Frequency vs Capacity of Tuned Circuit of Oscillator
II. Change in Frequency vs Second of Condenser Shunting Crystal.

The conductor plates are made very rigid in order to keep the separation constant, since a variation in this spacing as small as 0.01 mm would cause an appreciable error in frequency. The temperature coefficient of frequency is about 0.00038 per cent per degree Centigrade, requiring a control of temperature constant to within 0.025° C in order to keep variations due to temperature change alone within one part in ten million. Measurements on the effect of atmospheric pressure, covering the range from 760 mm to very low pressures, show the frequency to be approximately a linear function of the pressure. For the particular crystal measured, it was found that an increase in pressure of 140 mm caused a decrease in frequency of 0.001 per cent.

The thermostat used at present to control the temperature of the crystal is shown in Fig.7. It consists of a steel cylinder with hollow walls filled with mercury which expands when heated into a capillary tube, as in a thermometer. At a certain temperature contact is made on a pointed tungsten wire (not shown) which operates a mechanism for changing the rate of heating applied through a resistance coil wound on the outside of the steel cylinder. The crystal is mounted within the cylinder and the whole device is enclosed in a balsa-wood box for heat insulation. By means of this arrangement the temperature at a point within the cylinder may be kept at a prescribed value over long periods.

It is of interest to note that the temperature of the crystal when operating is not exactly that of the space surrounding it because some energy is dissipated within the crystal. For this reason, since the dissipation varies at least as the square of the applied plate voltage, that voltage is kept at a low value. The resistance, R, in the circuit of Fig. 3 is for this purpose.

The vacuum tube circuit of the oscillator with the crystal and temperature control apparatus is shown in the photograph Fig. 8. This whole circuit is mounted in a closed cabinet which itself is temperature controlled by a commercial thermo-regulator.

An important element of this new frequency standard is the submultiple frequency generator which is used as the first element of the system for comparing the rate of the crystal controlled oscillator with the time standard. This submultiple producer consists of an oscillator for generating the low frequency, a harmonic producer for obtaining a harmonic of the low frequency which corresponds to the high frequency by which it is controlled, and a modulator from which is obtained a direct current the amplitude of which is a function of the phase relation between the controlling high frequency current and the harmonic of the controlled low frequency current. The action of the direct current on a control element in the low frequency oscillator maintains the frequency at such a value that the current from the harmonic producer has exactly the same frequency as that from the standard oscillator. Fig. 9 shows the circuit arrangement. The tubes, from the left, are associated with the modulator, the oscillator, the harmonic producer and an output amplifier, respectively.

The control device consists of a core of magnetic material having two windings, one in the oscillating circuit proper and one in the plate circuit of the modulator tube. This coil is designed so that a variation in the direct current output of the modulator causes a variation in the inductance of the winding in the oscillating circuit by virtue of the magnetic saturation of the core.

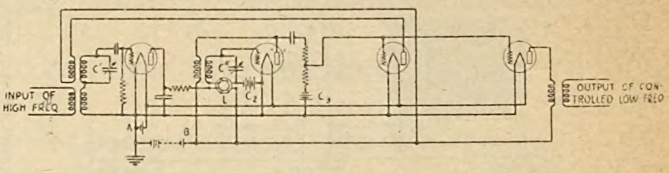

FIG. 9. — Submultiple Controlled Frequency Generator.

In operating, the low frequency oscillator is adjusted so that the frequency is exactly some submultiple of the control frequency when the direct current into the control coil has a certain mean value. If, then, anything occurs that tends to change the low frequency, the resulting phase shift between the harmonic and the control current instantly causes a change in the direct current in the control coil that opposes that tendency. The result is that, in spite of large variations in the low frequency circuit, the frequency is maintained at an exact submultiple of the high frequency control, the only variation being a slight shift in phase with respect to the control current.

With a circuit operating in this fashion it is possible to control a current so that its frequency shall be maintained in an exact integral relationship to that of some other current, even when the latter is many times higher. A ratio of 50 to 1 may be readily secured with moderate care. It would thus be entirely feasible to secure, by repeated stages, a current having a frequency which was exactly one five-hundredth the frequency of the 50,000 cycle standard, or 100 cycles. This current could then be used to run the synchronous motor of the clock previously described. Actually it has been found advantageous to secure a current having a frequency one-tenth that of the standard and to use this current to operate a 5,000 cycle synchronous motor.

This high frequency synchronous motor operates on the output of a single 5 watt tube.

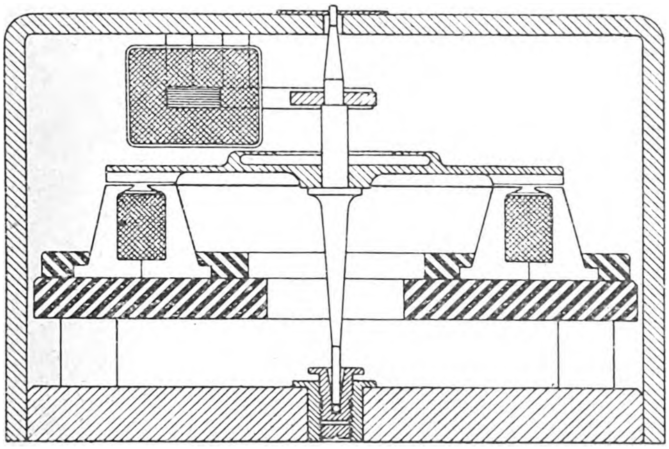

Fig. 10. — Sectional View of 5,000 Cycle Synchronous Motor.

The rotor is mounted on a vertical steel shaft running on a single steel ball under oil. The motor is mounted under a bell jar and is operated in partial vacuum in order to reduce friction losses and to keep the bearings clean. It takes more than half an hour for the motor to come to rest from full speed when it is disconnected from the source of power.

The rotor is a flat disc having 100 teeth milled on one side for poles and having an annular recess in the upper side for the mercury used to prevent hunting. The stator consists of 100 U-shaped pieces of silicon steel arranged in radial planes equally spaced around the axis. These form a ring of the same outside diameter as the rotor, and having an annular space in which is located a coil for magnetizing all the poles in parallel. A single U-shaped piece and a single fin on the rotor constitute one magnetic circuit around the coil. By this construction the minimum magnetic material is used and it is possible to laminate properly every part. Coils can be wound to match the electrical impedance of any suitable vacuum tube and can be exchanged easily. The spacing between the rotor and stator can be altered for any special case by adjusting the lower bearing. Figs. 10 and 11 show a sectional drawing and a photograph of the assembled motor.

The coil of the motor is connected in the plate circuit of the tube used to drive it so that both alternating and direct current flow in the winding. This produces 5,000 magnetic pulses per second instead of 10,000 as would be the case if alternating current alone were used. Thus the rotor moves one pole per complete cycle and revolves at 50 revolutions per second.

Maximum power is obtained in the motor if its impedance is conjugate to the impedance of the output circuit of the amplifier. The internal output impedance of a tube is nearly pure resistance, so the reactive component of the motor impedance is balanced by a series condenser between the motor winding and the filament. The space current is supplied through a choke coil connected to the junction of the motor winding and the condenser. A somewhat better arrangement would be to resonate the motor winding by a parallel connected condenser and to connect the plate battery at a tap in the winding. In this way the choke coil can be dispensed with and the ratio of alternating to direct current in the winding can be adjusted so that the resultant goes to zero once during each cycle. At the same time the impedance of the tube can be matched exactly, as required for maximum power transfer.

It would be a simple matter to arrange a gear train so that this motor might operate the clock mechanism directly. Since, however, there were already available clocks designed to operate on a 100 cycle current, it was thought to be simpler to use these as they were. Consequently, the high frequency motor drives a small generator, the two-pole rotor of which is mounted on the same shaft. Since the speed of the motor is

50 r. p. s., and since the generator is of the inductor type, the generated frequency is 100 cycles — or exactly 1/500th of the original high frequency. The output of this generator is amplified by a single vacuum tube which supplies sufficient power to operate the motor of the 100 cycle clock, the rate of which, therefore, is an accurate measure of the frequency of the 50,000 cycle oscillator.

In the design of the electrical circuits, provision is made for supplying power from multiple amplifiers at 50,000, 5,000 and 100 cycles, which frequencies are in exact harmonic relation. These are available continuously for making frequency comparisons in the three ranges.

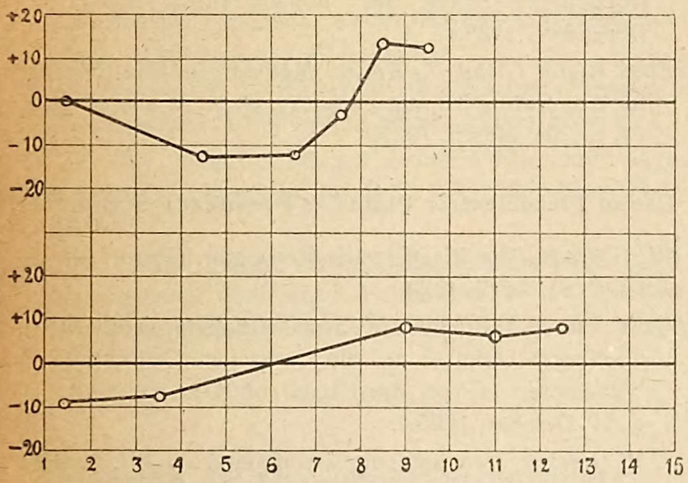

Fig. 12. — Daily Variation in Average Frequency of 50 Kilocycle Standard.

In addition to this provision, circuits have been built that permit the control of any frequency which is a multiple or submultiple up to the tenth order of any one of the three original frequencies. Also, many fractional multiples may be thus controlled. This considerably extends the usefulness of the new standard since it is possible to deliver alternating current at constant frequency over a range from 10 cycles to 500,000 cycles per second. These frequencies are not only constant but are known to the same accuracy as the 50,000 cycle control frequency, being some predetermined exact rational multiple or fraction of that frequency.

Because of the short time during which this standard has been in operation, and because of numerous interruptions for the purpose of making slight modifications, data covering a long continuous run are not available. Measurements made over several brief periods are, however, of interest as they indicate the performance which may be expected from the system. Fig. 12 gives the results of two brief runs. On this chart each point represents the deviation of the average rate for a single twenty-four hour interval from the mean rate for the entire period. It is probable that variations in atmospheric pressure were responsible to a considerable extent for the variations in frequency. It is obviously desirable to provide means whereby the crystal may be operated under constant pressure as well as constant temperature.

The above data indicate a high degree of constancy of the average frequency of this standard over long time intervals. In view of the very great ratio between the rate of the standard and the mean solar day, it is desirable to obtain additional checks as to the constancy of frequency over shorter time intervals. For this purpose a second oscillator, similar to that already described, was adjusted to approximately the same frequency, the actual difference being 0.2 cycles per second. Data taken by measuring the duration of ten consecutive periods at half-hour intervals show the relative variations. The results of these measurements are given in Fig. 13. Each point represents the difference between the ratio of the rates for any one interval from the average ratio of the

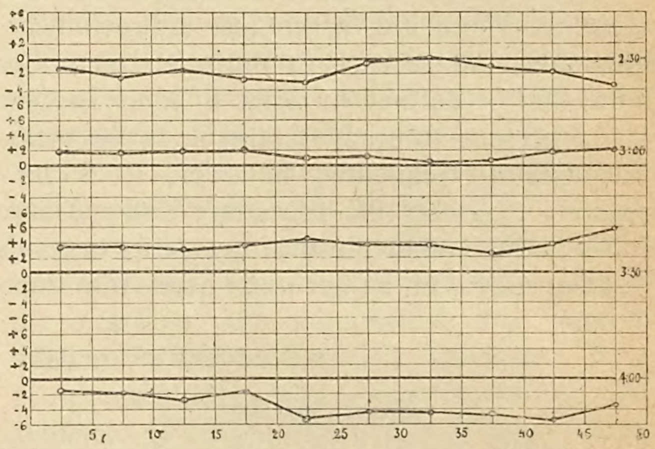

Fig. 13. — Relative Variation in Frequency of Similar Crystal Controlled Oscillators.

rates for the entire two-hour interval. It will be noticed that for some of the runs, particularly that made at three o'clock, the ratio of the rates remains practically constant. At other times, however, as at two-thirty and three-thirty, there is a marked periodicity to the variation. This may be accounted for in terms of temperature variation; the periodicity of the variation is practically the same as that of the operation of the thermostats. Whenever the two thermostats are operated out of phase, there is a noticeable change in the ratio of the rates; whenever they are operated in phase, their rates remain practically constant.

In view of the fact that the accuracy to which a frequency may be defined depends on its constancy, on the precision of comparing its rate with the rate of the earth's rotation, and on the accuracy to which the latter rate can be determined, it seems reasonable, in view of the above data, to expect that in the near future it will be possible to define a frequency at any instant to an absolute accuracy of at least one part in ten million.

It has not been unusual to determine frequency in terms of some other quantity such as the acceleration due to gravity, the velocity of light, or the product of inductance and capacity. These are, of course, indirect measurements because each of the other quantities must first be defined in terms of time. In view of the present accuracy obtainable in the measurement of frequency, however, it is now possible to invert the procedure and measure some of these other physical quantities in terms of frequency with an accuracy heretofore unattainable.

November 16, 1927.

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* * *

Ionization in the upper atmosphere (1),

by E. O. HULBURT,
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The more important agencies which may conceivably cause the ionization of the upper atmosphere of the earth are the ultra-violet light, α and β particles, all of solar origin, the penetrating radiation of cosmic origin, and the ionizing radiations from terrestrial sources. The last mentioned may perhaps be ruled out immediately because of the fact that the conductivity of the lower atmospheric strata increases rapidly with the height for the first few kilometers. The possible effects of these ionizing agencies have been considered in papers by Chapman and Milne, Benndorf, Elias, Lassen and others. Recently experiments with the electromagnetic waves of wireless telegraphy together with theories of the propagation of these waves over the surface of the earth have lead to information about the ionization in the upper atmosphere more definite than hitherto obtainable, and it has been of interest to examine again the causes of the ionization.

The experiments of Breit and Tuve with 70 meter waves, of Appleton with 400 meter waves, of Heising with 57 and 111 meter waves, of Wagner and Quack with 15 and 16 meter waves, of Hollingsworth and Eckerslay with long waves, and the experiments of Taylor on the skip distances of waves below 50 meters and the theoretical considerations of Taylor and Hulburt on these skip distances, show that the electron density N increases with the height Z above the earth reaching a value of about 4×10^5; above the height where N has this value the electron density is not known except that it does not go on increasing. Although the wireless data are none too extensive it may be taken that in the day-time (for the North Temperate Zone) the height where N is of the order 10^5 is roughly 150 kilometers, and where N is 10^3 is about 60 kilometers.

Because of the diurnal variation in the ionization we choose the ultra-violet light of the sun as being the ionizing agency deserving first consideration. In order to make an explicit calculation of the ionization the temperature, the pressure, and the constituent gases and their partial pressures must be known at each height in the upper atmosphere. These we may take as given completely in the classical thermodynamic isothermal equilibrium theory of Humphreys, Jeans and others. There is a question as to the existence of hydrogen in the upper atmosphere, but the conclusions given later are much the same whether hydrogen is there or not. The law of the recombination of the electrons with the positive ions must be known. For this we have J. J. Thomson's theory of recombination, and complete formulas are available. We must also recognize the possibility of the electron attaching itself to a neutral molecule, for when it does this, thereby producing a negative ion, it is no longer an energetic refractor of the wireless rays. The oxygen molecule is the only important one in this connection, and the values of the attachment coefficient measured in the laboratory for pressures of 10 mm. of mercury and above must be extrapolated to pressures below 10^{-2} mm., perhaps a questionable extrapolation. The diffusion of the electrons and ions must be considered, this is a very important influence. Using all these things it is concluded that the solar ultra-violet light is a necessary and sufficient cause of the Kennelly-Heaviside layer, and that hypotheses of other agencies of ionization, such as charged particles from the sun, penetrating radiation, etc., are uncalled for except perhaps in unusual cases. Assuming the solar ultra-violet energy to be that of a black body at 6000°K and using known, or estimated, absorption coefficients of the atmospheric gases for ultra-violet waves below λ1300A which cause photo-electric ionization, the electron curve for the sun overhead, i.e. for high noon in summer in the temperate zone, has a maximum of 3×10^5 electrons per cm^3 at a height of 190 km. Below this the ionization can not be calculated exactly, because of the lack of many facts which future laboratory experiments may supply, but a density of 10^4 to 10^5 electrons per cm^3, or 10^9 to 10^{10} ions per cm^3 (or a suitable mixture of ions and electrons) seems possible down to, say, 100 km. This ionization is shown to explain quantitatively many facts of wireless telegraphy, i.e., the skip distances, overhead absorption coefficients, limiting waves, ranges and the apparent heights reached by the waves. Taking into account the seasonal changes in the upper atmospheric pressures and the altitude of the sun, the electron layer for winter noon is found to have a maximum of 1.42×10^5 at a height 147 km, and the ionization below this is less than the summer values by a factor of 2 or so. After sunset the maximum ionization is found to decrease by a factor of about 6 in the small hours of the morning; below the maximum the decrease is greater. These seasonal and diurnal changes in the ionization are shown to be in agreement with the corresponding variations in wireless wave propagation phenomena.

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(1) Abstract of a paper read at the open meeting of the U. R. S. I. at Washington, D. C., October, 1927.

The potential energy of the daytime ionization in a 1 cm^2 column of the atmosphere is found to be at least 1 erg and the assumption that less than 1 % of this is liberated as light will account for the light of the night sky, or the *non-polar aurora,* as Rayleigh calls it. Due to diffusion of the ions along the lines of magnetic force the upper ionic spray of the ionized layer diffuses to the magnetic poles, concentrates there and causes the *aurora.* Calculation indicates that the *ozone* of the atmosphere is perhaps not directly connected with the foregoing ionization, but is formed by longer wave-lengths of ultra-violet light from λ1300 to 1800A.

* * *

An automatic recorder for measuring the strength of radio signals and atmospheric disturbances,

by E. B. JUDSON,
Laboratory for Special Radio Transmission Research,
Bureau of Standards,
Washington (D. C., U. S. A.)

The recording system described was designed for the purpose of obtaining knowledge of the behavior of radio signals over considerable periods without the presence of an observer. With the apparatus used it is possible to obtain hourly observations of the field strength of several stations and atmospheric disturbances throughout the twenty-four hours.

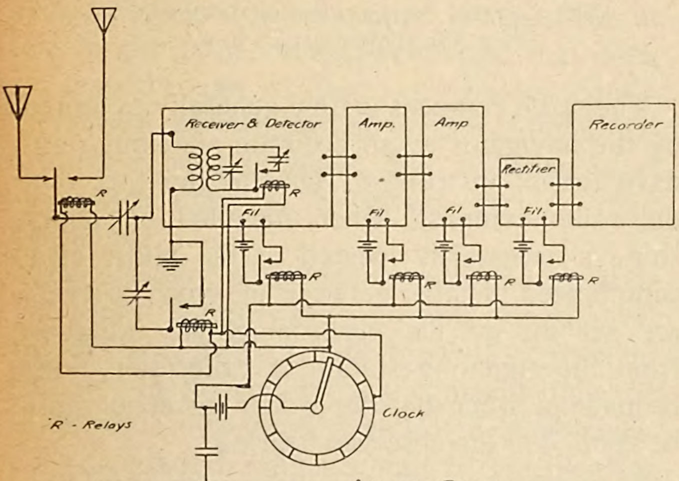
The entire apparatus is controlled by a clock, which, at different five-minute periods during the hour actuates a series of relays automatically turning on the receiving set and amplifiers and tuning to the desired stations.

Figure 1 shows the schematic diagram of the circuit and figure 2 shows the arrangement of the apparatus.

The receiving set is of the conventional autodyne type with two tuned circuits having a tuning range from 60 kc. (5,000 m.) to 12,0 kc. (25,000 m.). Two antennae are used, one having an effective height of 16 meters for reception of transatlantic stations and atmospheric disturbances, while the other having an effective height of $1^{m}5$ is used for nearby American stations.

In order to insure greater constancy of sensitivity, only audio-frequency amplification is used. This consists of two stages of transformer coupling followed by four stages of resistance-capacity coupling. With this arrangement, as long as all the filament currents remain constant and the plate voltages do not change the system retains its calibration with sufficient accuracy (within 10°/₀) over periods of several months.

The last audio-frequency amplifier is coupled to a rectifier circuit, containing the recording galvanometer, through an air core audio-frequency transformer. A three-electrode tube having the plate and grid connected is used for rectification. Generally, tubes used in this manner give a slight galvanometer deflection, when no signal is present, due to the initial velocity of the electrons. This current, however, can be balanced out by a reversed E. M. F. across the galvanometer if it is comparable to the current received from signals.



Automatic Recorder -
Laboratory for Special Radio Transmission Research - Bureau of Standards.

FIG. 1.

Records of the variation of signals and atmospherics are made with a Cambridge-Paul Thread Recorder.

This instrument is essentially a sensitive recording galvanometer having a moving coil arranged to give a series of instantaneous records of the galvanometer deflections. The moving coil of the galvanometer has attached to it a pointer which overhangs the drum carrying the paper, while between the pointer and the drum an inked thread is stretched parallel to the axis of the drum at a short distance above its surface. A presser bar is situated above the galvanometer pointer which is normally held free of the pointer by a cam and its follower. At regular intervals the cam makes a half revolution, first allowing the presser bar to fall upon the pointer and immediately raising it to its normal position. As the presser bar falls it depresses the pointer on the drum nipping the inked thread between the pointer and the paper producing a dot on the paper, which makes a visible record of the deflection of the galvanometer at the moment. The drum of the recorder is arranged for two speeds, one giving a complete revolution in twenty-four hours and one giving a revolution every two hours for short time records. For the present purpose the twenty-four hour revolution is used.

The clock controlling the entire system has its face plate equipped with twelve brass segments arranged in a circle, each segment representing five minutes. A spring on the end of the minute-hand makes an electrical connection with each segment as it passes over.

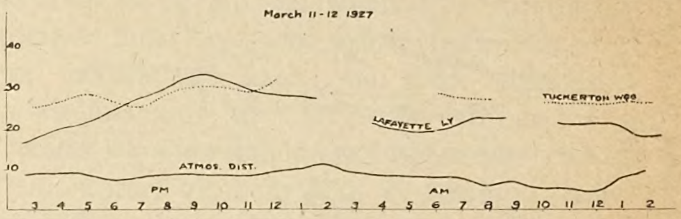


FIG. 3.

The cam allows the presser bar to depress once every thirty seconds, making ten dots on the paper for one five-minute segment.

For the present purpose, the first five-minute segment in the hour is connected so that when the minute-hand contact passes over it, a relay closes, turning on the receiving set, tuned to the station having the highest frequency of those to be measured. The amplifiers, the rectifier tube and the presser bar control are switched on simultaneously by means of other relays.

The following segments operate the same relays as the first, but are arranged to close other relays which connect parallel variable capacities across the primary and secondary tuning capacities of the receiving set. This allows the receiver to be tuned to other frequencies effective only for a certain clock segment.

The entire system is slightly sensitive to change of tone. The pitch of the signals being recorded is therefore set to 1,000 cycles by comparison with an electrically-driven 1,000-cycle tuning fork. A 30 ohm damping resistance across the galvanometer coil slows its period so that it is little affected by change of speed in transmission. Absolute calibration of the receiving set is obtained from a radio-frequency oscillator feeding into the antenna circuit, while the amplifiers and rectifier may be checked at any time by connecting them to the output of the telephone comparator (1).

The deflection-current curve of the galvanometer is nearly straight over the usual range of observations. As a protection against heavy atmospherics the deflection is limited just below full scale by limiting the output of the last amplifier tube.

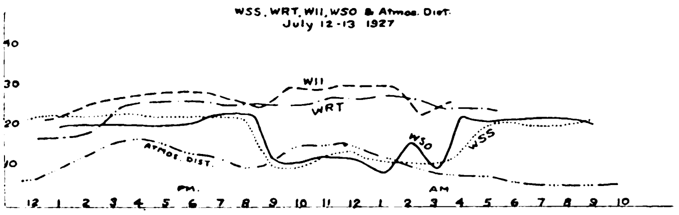


FIG. 4.

Continuous records of several stations and atmospheric disturbances have been made at the Laboratory for Special Radio Transmission Research of the Bureau of Standards since February, 1927. During the winter months when atmospheric disturbances were low, records were made of Lafayette (LY) and Rugby (GBR) along with Tuckerton (WGG) or Rocky Point (WSS). During the heavy static season, measurements were confined to four American stations, namely, two New Brunswick stations (WII and WRT), Rocky Point (WSS) and Marion (WSO), reception being on the small antenna. The large antenna, however, is used for reception of atmospheric disturbances.

Figure 3 shows a typical twenty-four hour record of Lafayette (LY), Tuckerton (WGG) and atmospherics at 12 kc. (25,000 m.).

In figure 4 are given typical twenty-four hour summer curves, of WSS, WRT, WII, WSO and atmospherics which show in the case of WSS and WSO an apparent interference of the reflected and direct waves during the night.

* * *

Experiences in Radio Compass Calibration,

Abstract of paper by F. A. KOLSTER,

Chief Research Engineer, Federal Telegraph C°,
Palo Alto (California, U. S. A.)

The radio compass is now generally accepted by the navigator as an accurate and important navigational instrument. Due to the fact that the radio compass, when installed on board ship, is necessarily located in the midst of a complicated metallic network in which currents are set up by the electromagnetic radiation from the signaling station, a calibration must be made in order that the proper correction may be applied to the observed bearing.

Deviation from the true bearing is caused by the effects of the metallic mass of the ship and experience has shown that this deviation follows very closely a sine curve, assuming positive values over the starboard bow and port quarter and negative values over the port bow and starboard quarter.

Accuracy is dependent upon the permanency of the calibration and it is important, therefore, that the large conducting system, made up of masts, stays and rigging, remains the same at all times. Long stays should be thoroughly insulated to avoid large electrically closed loops and mast guys should be well bonded to the ship's hull to avoid varying electrical effects.

This problem should be understood by naval architects and ship builders in order that proper consideration may be given to the rigging of ships from the point of view of the resulting effects upon the performance of the radio compass.

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(1) *Proc. I. R. E.*, vol. 12, n° 5, p. 521; 1924.

A theory of the upper atmosphere and meteors (1),

by H. B. MARIS,

Naval Research Laboratory, Washington (D. C., U. S. A.)

The force of gravity acting on the atmosphere of the earth causes the heavier gases to settle downward and the lighter gases to rise to higher altitudes by diffusion and winds unhindered by diffusion would by convection keep the composition of the air uniform at all elevations. The classical ideas of the change in atmospheric pressure with altitude (e. g. Humphrey, Jeans, Chapman and Milne, etc.) have been based on the assumption that convection is negligible, at least in the upper atmosphere, and that each gas was through diffusion in gravity equilibrium with its own partial pressure. Investigation has shown, however, that diffusion is of importance only at elevations greater than 100 kilometers.

The ordinary equations of diffusion show at once that if the air were uniformly mixed at all altitudes and then left free from all convection currents, there would be a constant flow of lighter molecules upward and of heavier molecules downward, which would be independent of the altitude until a level was reached where the diffusing gas would be in gravity equilibrium. This « diffusion » level for hydrogen would move from infinity down to 142 kilometers in one day, at the end of five days it would be at a height of 127 kilometers and in 50 days it would be at 113 kilometers. The corresponding levels for helium would be at 137, 120 and 106 kilometers respectively. The new calculations give hydrogen and helium contents above 150 kilometers roughly 1/100,000 of the values previously calculated.

Recent use of the upper atmosphere as a medium for transmitting electromagnetic radiation of wavelength 10 to 10,000 meters has emphasized the importance of knowledge or at least of a theory of changes which occur in the upper atmosphere between day and night conditions during different seasons of the year. Absorption of solar and terrestrial radiation by the air must determine any theory of temperature distribution in the upper atmosphere. Humphrey has discussed this problem and has suggested that it should have been worked out but no attempt has been made previously to apply radiation and absorption coefficients and solve for the thermal condition of the upper atmosphere or to estimate probable temperatures at elevations greater than 20 kilometer for the radiation conditions of day and night or winter and summer.

Water vapor above 11 kilometers absorbs a little over 20 % of black body radiation from below at earth temperatures while carbon dioxide absorbs nearly 40 %. Ozone absorbs only about 2 % but its presence is important because it absobs about 4 % of the solar radiation at an altitude where most of the reradiation must be by the ozone itself. Temperature calculations based on these absorption coefficients show that for a 50° latitude above a height of sixty kilometers we should expect a temperature of about 250° K during a winter day with a drop to 220° during the night and a temperature of 370° during a summer day with a corresponding drop to 230° during the night. The atmosphere at the base of the stratosphere cannot be in radiation equilibrium but must receive more radiant energy than it looses both from above and below during a twenty-four hour day. The loss in heat by radiation from the warm equator is much less than from the cooler polar regions. An increase in temperature at sea level near the equator would not result in an increase in the energy lost by radiation from these regions, but would actually result in a decrease. Loss of heat by radiation from the earth depends, not on the condition of the surface, but on the temperature at the base of the stratosphere and absorption in the stratosphere. The temperature condition of the earths surface is in very unstable equilibrium. A slight change in the carbon dioxide of the air would have a termendous influence on the climate of the earth. If the carbon dioxide content of the air were increased from the present 0.03 % to 0.1 % tropical plants would probably grow in the polar regions. On the other hand if this protecting sheet decreased from 0.03 °/o to 0.01 °/o ice would probably be found near the equator.

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(1) Abstract of a paper read at the open meeting of the U. S. R. I. at Washington, D. C., October 1927.

Since the present theory leads to low densities of the atmosphere above heights of 100 kilometers, densities one hundred thousandth of those of classical tables at 300 kilometers, the facts about the appearances of meteors require explanation. It seems possible to do this following to a certain extent the ideas of Sparrow and to a certain extent those of Lindemann. When a high speed meteor strikes an air molecule, it is assumed that the energy of the impact violently ejects atoms, molecules and possibly small particles of molecular dimension from the body of the meteor. This ejected material by virtue of its velocity carries into the air the energy which eventually gives the light of the meteor trail. For example, when a nitrogen molecule strikes an iron meteor which has a velocity of 40 kilometer per second, the energy of the impact is sufficient to raise the temperature of 1,800 molecules 1,000° C or to evaporate 56 molecules of iron, or to evaporate and ionize 24 molecules of iron. As a result of this impact a mass many times that of the nitrogen molecule is ejected from the meteor principally in the form of highly energized iron atoms which have velocities slightly greater than that of the meteor itself. The inelastic collisions of these iron atoms with the molecules of the air result in the visible trail. The excitation energy of these collisions may be as high as 155 volts for nitrogen or 280 volts for argon. Much of this energy may be radiated in the ultra-violet or even soft x-ray region, and it is probable that not more than one-tenth of the total radiation is in the visible part of the spectrum. Therefore, the total mass of the meteor must be much more than that derived by Lindemann and Dobson from their considerations of the relation between the mass of a meteor and its light. The temperature changes in the upper atmosphere from evening to morning, and from winter to summer, given by the present theory lead one to expect appearance of meteors at heights which are greater by say 5 kilometers in the evening than in the morning and in the summer than in the winter. It would be interesting to know whether this difference has been observed.

Recent studies of the propagation of electromagnetic waves over the earths surface have emphasized the need of a theory and definite conclusions concerning diurnal and seasonal changes in temperature and composition of the atmosphere at heights greater than 50 kilometer. It was the purpose of this discussion to take what steps were possible toward the meeting of this need.

Naval Research Laboratory,
« Bellevue », Washington, D. C.
1 November 1927.

* * *

Note au sujet des ondes de quelques mètres,

par René MESNY.

Dans ces dernières années, nous avons eu l'occasion de faire, en collaboration avec MM. Beauvais et David, une série d'expériences avec des ondes de longueurs comprises entre 2 et 4 mètres. Ces expériences ont permis de fixer les propriétés de ces ondes et leurs conditions d'utilisation; nous les résumons ci-dessous.

Emission et réception. — Le générateur a toujours été du type symétrique (1) à deux triodes; la puissance d'alimentation a varié entre 2 et 200 watts.

L'antenne était le plus souvent demi-onde, mais nous avons aussi employé des antennes de plusieurs demi-ondes et même des antennes absolument quelconques comme celles des navires sur lesquels nous avons travaillé.

La réception a toujours été une superréaction dont l'emploi s'est montré avantageux et pratique.

Propagation loin du sol. — Entre deux points qui se voient l'un l'autre, les transmissions sont excellentes à toutes distances, même avec les plus faibles puissances. Dans les Alpes, entre deux sommets dont la distance était de 100 kilo-

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(1) R. Mesny, *Les Ondes électriques courtes*, p. 61.

mètres, nous avons réalisé des communications téléphoniques duplex parfaitement sûres avec des postes portatifs à piles dépensant 3 watts pour l'alimentation de l'émetteur. Dans d'autres circonstances, nous avons atteint jusqu'à 280 kilomètres, la réception restant encore extrêmement forte quoique l'émetteur et le récepteur ne soient plus tout à fait en visibilité.

Il n'y a d'ailleurs rien qui puisse étonner dans ces résultats; ils sont dus tout simplement à ce que l'énergie est parfaitement utilisée pour le rayonnement avec ces ondes très courtes. Si W est l'énergie rayonnée en watts et D la distance en kilomètres, le champ à cette distance est, en microvolts par mètre,

$$\mathcal{E}^{\frac{\mu V}{m}} = 6700 \frac{\sqrt{W^{w}}}{D^{km}},$$

en supposant que l'émetteur soit isolé dans l'espace et qu'il n'y ait pas d'amortissement. Un seul watt rayonné donne donc, à 100 kilomètres, un champ de $67 \frac{\mu V}{m}$.

Influence des écrans. — Quand il existe un écran entre l'émetteur et le récepteur, les signaux sont d'autant plus atténués que la hauteur de cet écran est grande, et ils ne tardent pas à s'annuler quand elle croît. Nous avons fait plusieurs expériences en pays de montagnes en intercalant entre l'émetteur et nous-même l'écran formé par une chaîne de collines situées à des distances de 20 à 30 kilomètres. En nous déplaçant sur une pente opposée à l'émetteur nous pouvions, soit nous mettre en visibilité de ce dernier, soit pénétrer plus ou moins profondément dans son ombre géométrique. Dans tous les cas, les signaux ont disparu assez brutalement au moment où la ligne joignant le récepteur au bord de l'écran faisait avec celle qui allait directement à l'émetteur un angle de 1° à 1°,5. Aucune mesure quantitative n'a encore pu être faite, et la rapidité avec laquelle on voit disparaître des signaux forts dépend beaucoup des récepteurs employés; cependant, nous avons l'impression que la diminution des signaux était plus brutale que ne l'indiqueraient les formules d'optique relatives à la diffraction par un écran rectiligne.

Propagation près du sol. — Dans le voisinage d'un sol plan les choses peuvent complètement changer d'aspect.

Tout d'abord si l'émetteur et le récepteur sont tous deux à quelques mètres d'un sol plan, ou à peu près plan, sur une grande étendue, les communications faites avec une puissance de 3 watts et qui étaient excellentes à 100 kilomètres entre deux sommets, deviennent médiocres à 1,5 kilomètre; les signaux disparaissent complètement entre 2 ou 3 kilomètres.

Mais, il n'est pas nécessaire que l'émetteur soit près de ce plan, il suffit que le récepteur le soit. Nous avons fait de nombreuses expériences en plaçant l'émetteur au sommet de la tour Eiffel et en nous promenant avec le récepteur dans la région parisienne. Chaque fois que nous nous trouvions sur un sommet les signaux étaient extrêmement forts à toutes distances. Au contraire, dès que le récepteur était sur un plateau, les signaux disparaissaient quand la distance était supérieure à 20 kilomètres environ. Si l'on allait se placer sur le bord du plateau situé du côté de l'émetteur, les signaux redevenaient forts; à 400 ou 500 mètres de ce bord, souvent même à 200 mètres, ils disparaissaient entièrement ou presque entièrement.

La première idée qui vient à l'esprit est de voir dans ce phénomène un effet de l'absorption du sol. La brutalité de l'affaiblissement quand on s'éloigne du bord de la falaise nous a fait chercher une autre explication. Il nous a paru difficile qu'en 400 ou 500 mètres l'énergie de l'onde puisse être absorbée à ce point. Les formules de Zenneck invoquées souvent en la circonstance ne nous paraissent pas applicables: elles concernent une onde qui a pris un régime permanent à la suite d'un parcours obligatoirement long au voisinage du sol, assez long pour que le champ électrique ait acquis une composante horizontale importante, pour que l'onde se soit inclinée en avant (cette dernière image est d'ailleurs peu précise en raison de la rotation du champ). Si l'on peut chercher à justifier une telle explication dans le cas où émetteur et récepteur sont près du sol, il n'en est pas de même quand l'émetteur est très élevé; comme il est en même temps relativement éloigné, on peut admettre que les ondes qui arrivent au récepteur sont des ondes planes, et quand ce récepteur est sur un plateau, on est en présence de la réflexion d'une onde plane sur un plan. C'est un problème classique et l'on trouve facilement que dans les conditions de la pratique (constante diélectrique et conductivité normales) la réflexion se fait avec changement de phase de près de 180°, si l'angle du sol avec les rayons est faible. Quand cet angle est d'un demi-degré, par exemple, le champ résultant à quelques centaines de mètres du bord d'un plateau est environ vingt-cinq fois plus faible qu'au bord où la réflection ne joue pas. Nous pensons que telle est la cause du phénomène observé.

Ce qui nous confirme dans cette opinion, c'est qu'après avoir déplacé l'émetteur et l'avoir installé sur un sommet d'une hauteur de 175 mètres (mont Valérien), nous avons pu obtenir des signaux encore très convenables, à plusieurs kilomètres du bord d'un plateau dont l'altitude était voisine de 200 mètres (au sud de Beauvais) et situé à une cinquantaine de kilomètres de l'émetteur. Il n'y avait plus d'onde plane réfléchie, mais un phénomène de diffraction. Les énergies en jeu étaient d'ailleurs du même ordre que quand l'émission se faisait du sommet de la tour Eiffel (150 watts alimentation environ).

Ces phénomènes n'ont peut-être pas d'importance pratique; ils paraissent néanmoins intéressants, en faisant ressortir différents modes de propagation.

Propagation aux grandes distances. — Des essais très soignés, exécutés dans des conditions très favorables, avec une puissance d'émission de 150 watts alimentation, ne nous ont donné aucun résultat à des distances de 600 et de 900 kilomètres. Quoique l'émetteur fût encore au sommet de la tour Eiffel, on était extrêmement loin de la visibilité théorique : L'horizon visible de la tour Eiffel est de 65 kilomètres seulement, et à une distance de 600 kilomètres la rotondité de la terre crée un écran d'une hauteur de 7 kilomètres. La réfraction ionique seule pourrait permettre de telles communications, et l'on sait qu'elle paraît insuffisante pour ramener au sol, même à des distances beaucoup plus grandes, des ondes d'une longueur inférieure à une dizaine de mètres.

Utilisation des ondes de quelques mètres. — L'utilisation pratique des ondes de quelques mètres est aussi simple que celle des ondes plus longues. Les petites puissances qui suffisent à couvrir les distances accessibles se réalisent très facilement; les antennes peuvent être quelconques, mais quand on emploie des antennes demi-onde on arrive à réduire l'encombrement dans des conditions fort avantageuses pour les usages pratiques. L'emploi de la superréaction pour la réception supprime toutes les difficultés qui pourraient provenir de l'élévation de la fréquence. Les atmosphériques sont absolument insensibles.

L'influence radicale des écrans donne à ces ondes une qualité de discrétion qui n'existe pour aucune autre gamme, et cette qualité peut être notablement accrue par l'emploi de systèmes directifs dont l'établissement est particulièrement facile.

Enfin, grâce à la réduction d'échelle, ces ondes permettent de faire très simplement de nombreuses expériences qui sont difficiles ou impossibles avec les ondes plus longues. Nous signalerons à ce sujet les mesures que nous avons effectuées sur divers systèmes directifs et en particulier sur les rideaux d'antennes en grecque. Cette disposition, imaginée par Chireix (1) et par nous-même (2), permet d'obtenir une concentration considérable d'énergie par les moyens les plus simples.

* * *

A Note on the Short Wave Long Distance Transmission,

by T. MINOHARA and K. TANI,
Naval Experimental and Research Establishment,
Tokyo (Japan).

It is well known that the path of propagation of short waves over long distances, depends on the position of the so-called Kennelly-Heaviside

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(1) Société Française Radioélectrique. — Brevet français, n° 216,757, 10 mars 1926.

(2) R. Mesny, Émissions dirigées par rideaux d'antennes en grecque. (*Onde électrique,* 6 mai 1927, pp. 181-200.)

layer which varies with day-time and season. Consequently, it is not simple to treat the phenomenon on the transmission over long distances, as the height of Kennelly-Heaviside layer is not the same at different localties where the time and season are quite different.

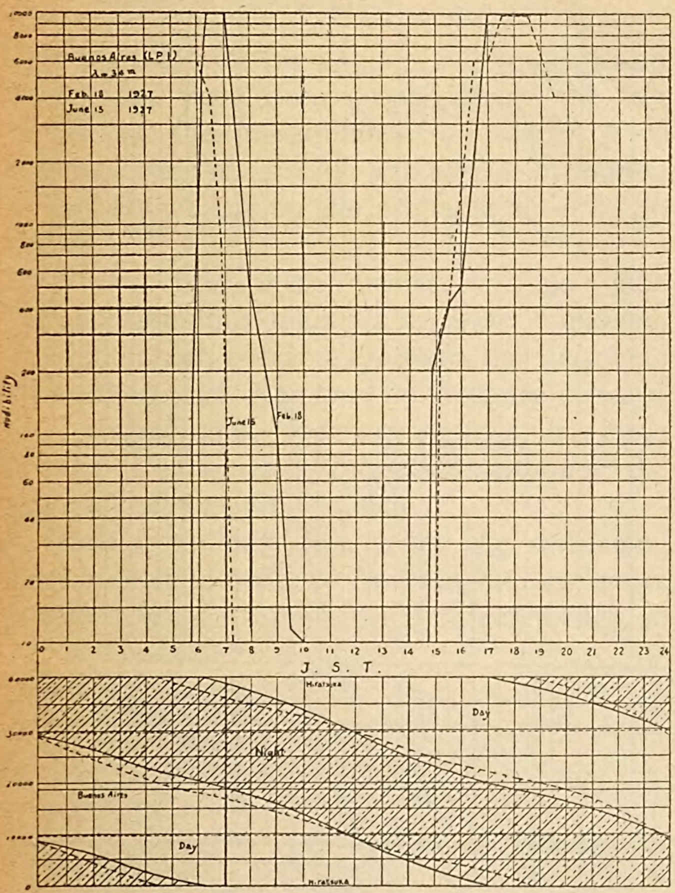

FIG. 1.

To study the experimental results of short waves in long distance communication, we take the following method which is very convenient to trace the course of the wave.

Plot the hours in abscissa, and the distance along the great circle between the stations in ordinate, as shown in the lower part of the Figs. 1 and 2.

Draw the boundary line of day and night. Then we can easily investigate the relation between the path of waves, and the darkened space.

Further we can suppose that the conductivity of the upper atmosphere as noon is not the same as right before the sun set or right after the sun rise. If we assume that the height of the conducting layer of the upper atmosphere is inversely proportional to the cosine of the angle of altitude of the sun at the locality to be considered, we can calculate and plot the curve representing the curvature of Kennelly-Heaviside layer as shown in Fig. 3.

Let us now explain some typical experimental data with the aid of the above consideration.

The Fig. 1 shows the test result of the signal intensities of short wave (λ = 34 m) sent by the station at Buenos-Aires (L P I), and received at our laboratory in Hiratsuka near Tokyo, on February 18th and June 18th 1927.

It is clear from the figure that the signals are strong twice a day in the period where the night signals travel along the shortest distance of darkened space in the opposite side of each other.

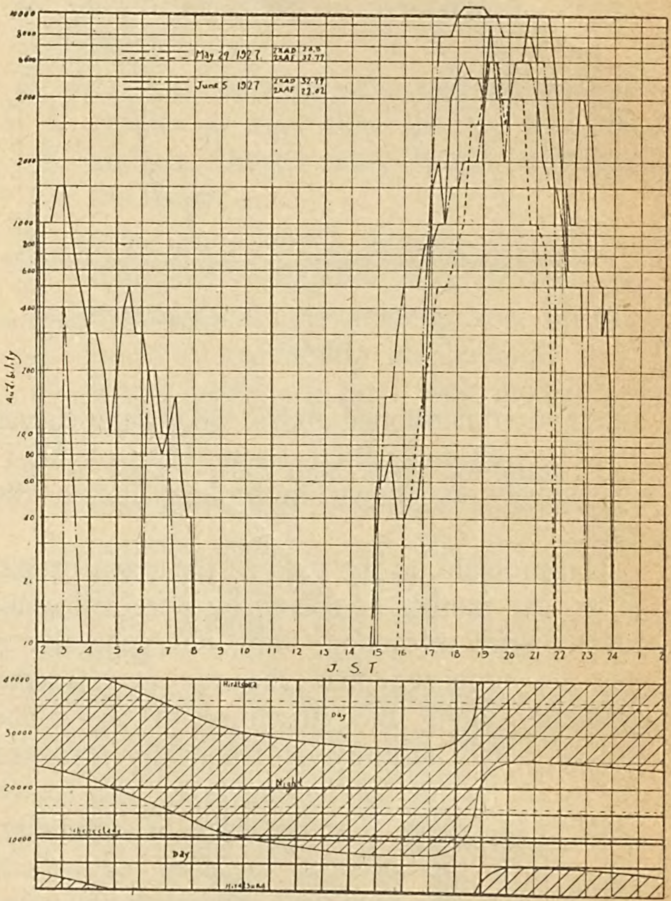

FIG. 2.

Fig. 2 shows the test result of the signal intensity of short wave sent by the station XAF Schenectady, and received at Hiratsuka.

It is a remarkable that the signals of the wave length 26.8 m are strong at 3.00 A. M.

and 6.30 A. M. J. M. T. In Fig. 3, ABA' represents the great circle containing the receiving station A and the sending station B developed into a straight line. *ll'*, whose height above ground is *h*, shows the level of Kennelly-Heaviside layer at the night-time, and DD', whose height is *h'*, shows the level of the layer at noon when the ionization is the most intense. Thus the conducting layer at any instant when the ray strikes at some angle of inclination, must lie between *ll'* and DD'. Plotting the position of the layer calculated under the assumption mentioned above, we get the curves of 3-00, 4-45, 6-30, 9-00, 14-00, 19-00, J. M. T. respectively as shown in broken line in the Fig. 3.

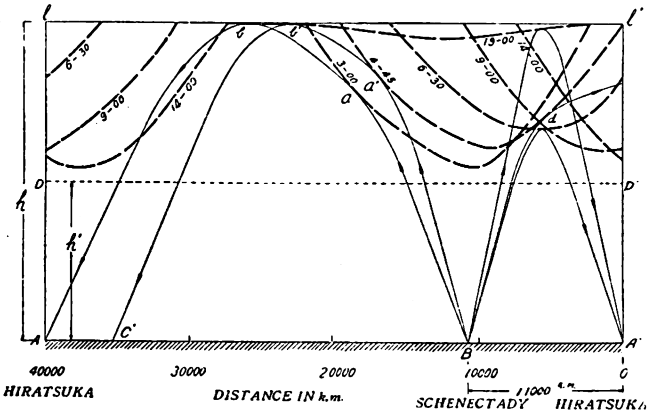
l
l'
6-30
9-00
14-00
b
b'
3-00
4-45
a'
a
6-30
9-00
19-00
14-00
d
D
D'
h
h'
A
C'
B
A'
40000
HIRATSUKA
30000
20000
DISTANCE IN k.m.
10000
0
11000 k.m.
SCHENECTADY
HIRATSUKA

Fig. 3.

The problem on the path of the wave propagation can neither be solved by pure reflection nor by pure refraction, only, but generally it may be supposed that the ray is refracted gradually along the curvature of the globe and refracted abruptly or practically reflected downward at the conducting layer and return towards the earth. For the sake of simplicity, we reduce the process to pure equivalent refraction in the graphical representation supposing the earth surface to be a plane. Then we get the trace of the path of the wave B*ab*A for 3-00 J. M. T. and B*d*A' for 6-30, J. M. T., therefore the signal can be received at the station A, but during the other period 4-45, J. M. T. for example, we can not receive the signal, as the wave travels along the path B*a'b'c'* or B*d*.

Fig. 4 shows the intensity of signal sent by the station Nauen, received at Iwatsuki. Similar remarkable fact can be seen in these results and the same explanation as above mentioned can be applied, as shown in Fig. 5.

As there are many assumptions and other unknown factors are involved in these treatments, it should be not so simple in actual case but they may be looked upon as new suggestions taking account the present knowledge of radio engineering.

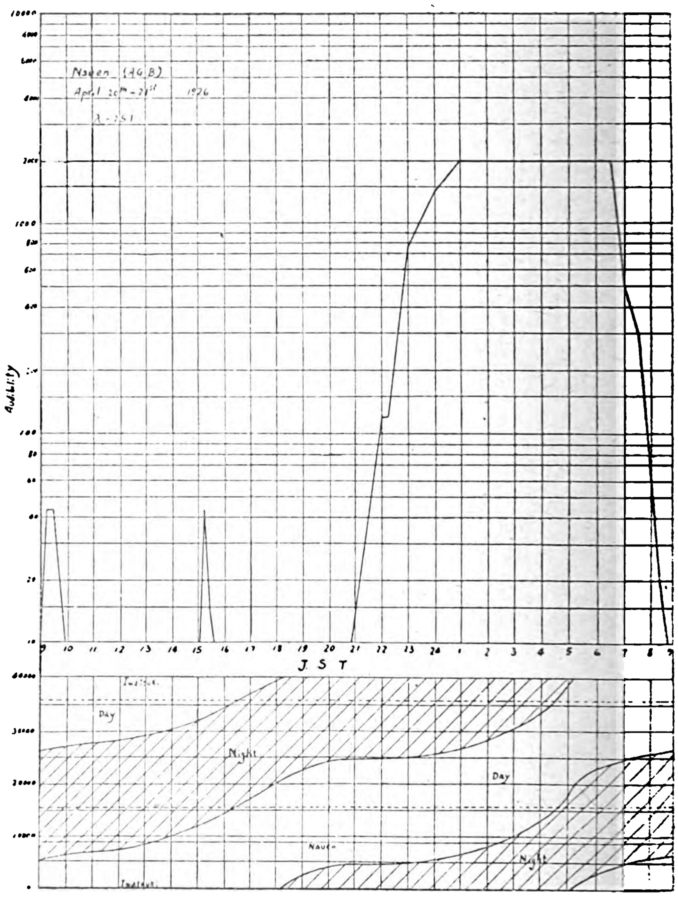

Fig. 4.

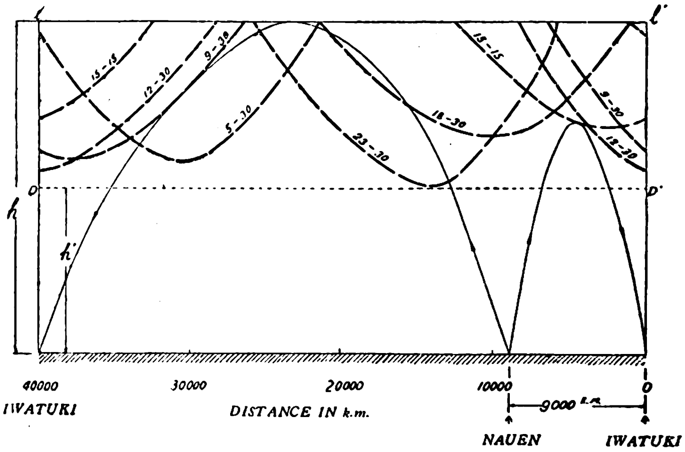
l
l'
15-15
12-30
9-30
5-30
15-15
18-30
22-30
5-30
18-30
D
D'
h
h'
40000
IWATUKI
30000
20000
DISTANCE IN k.m.
10000
0
9000 k.m.
NAUEN
IWATUKI

Fig. 5.

* * *

Diurnal Variation in Signal Strengths of Short Waves,

by T. NAKAGAMI and T. ONO,
Direction General of Telegraph and Telephone Engineering,
Ministry of Communications, Tokyo (Japan).

Many theories on short wave transmission phenomena have been proposed and the test results of the measurements on short wave signals reported, giving valuable contribution to our knowledge on the propagation of radio waves.

In examining these reports, we must take into consideration what waves were employed, in what season and at what hours the tests were carried out, and what was the distance between the sending and receiving stations, as these factors are intimately connected, although the mutual relations are complex.

During the past two years, we conducted several tests in order to find out the best wave length in the day-time or night-time transmission during some season for a certain distance in Japan.

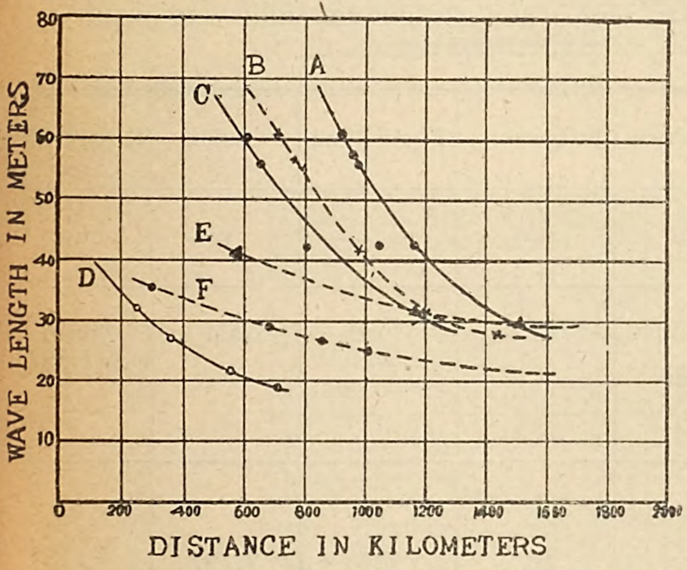

Fig. 2.

One of our tests was carried out at the beginning of January, 1927. A short wave transmitter was installed at Kagoshima Radio Station, and the antenna was of the simple V shape and its power about 500 watts, the natural wave length being equal to that of sending. The receiving stations were selected at different places, so that we could get sufficient data at different distances. At each place a short wave receiver consisting of a detector and one stage audio-frequency amplifier was installed.

We sent test signals during 24 hours on several different wave lengths at Kagoshima, and they were received at eight receiving stations simultaneously. Thus we got the results given in Fig. 1 which shows the diurnal variation in signal strengths with respect to the wave lengths and distances. From these curves we may observe the following interesting facts :

1. In night-time, the 61 m wave is unreadable at short distances, but as the distance becomes greater the signals get stronger and the hours of readability become greater.
2. The skip distance of a certain wave is greater in night-time than in day-time and the shorter wave skips more than the longer one.
3. As the wave length becomes shorter, the hours of readability in night-time get less and less at a short distance but the hours of readability on the same wave length become greater with increase in distance.
4. It seems that there are optimum day-time and night-time wave lengths at a definite distance and that the greater the distance, the shorter the optimum wave lengths. This will be described later.
5. Peculiarity in the transmission of the shorter waves along the meridian for a certain distance on a certain wave length is that the signals are unreadable except for a few hours at the sundrise and sunset. The reason of this fact seems that in the day-time the signals are absorbed and become unreadable owing to the low power and in the night-time the signals skip over this distance and at the sunrise and the sunset the altitude of the ionized layer or the degree of ionization in the upper atmosphere is favorable for the transmission on that wave length to that distance.

We also made several tests on short waves to find the optimum wave length for a given distance. One of the tests was conducted in June, 1926.

The test signals sent from Tokyo on different wave lengths were received simultaneously at several places, and we found out the wave length which can be received for 24 hours at a certain point, though this wave was neither the best one for the day-time nor for the night-time.

The result is given in the following table and in Fig. 2, Curve F :

Sending Station	Tokyo.				
Receiving Station	Kanagawa.	Hiroshima.	Sapporo.	Otchishi.	Kagoshima.
Distance between Sending and Receiving Stations in Km	300	360	890	965	965
Wave length in meters which gives all day signals	35	29	28	25	25

Another similar test was carried out in January, 1927, and we got the following result :

Sending Station. . .	Kagoshima.	Sapporo.	Kemikawa.	Kagoshima.	Kagoshima.	Osaka.	Kemikawa.
Wave length. . . .	61	75	56	4.28	41.5	32	27.5
Receiving point where the signals were readable throughout 24 h.	South of Naha. South of Kanazawa.	Hiraiso. Iwatsuki.	Hiroshima. Sapporo.	Iwatsuki.	North of Kanazawa.	Naha.	South of Naha.
Distance between Sending station and the Receiving point. .	700	780	800	985	900	1,170	1,550

Curve B, in Fig. 2 is plotted from the above table. Besides these, the other curves in Fig. 2 show the results obtained by similar tests carried out in Japan; i.e., Curve A is the day-time maximum range of the 500 watt transmitter in winter; Curve D, the skip distance of day-time; Curve C, that of night-time in winter; and Curve E, the wave lengths which may be used throughout day and night in spring. If we want to select the optimum wave length for commercial services, we must make use of Curves B, E and F in winter, spring and summer respectively, taking into consideration the fact that the optimum wave length at night is a few meters longer than that given by the curve, while the best wave length in day-time is a few meters shorter.

T. NAKAGAMI and T. ONO.

FIG. 1 - A

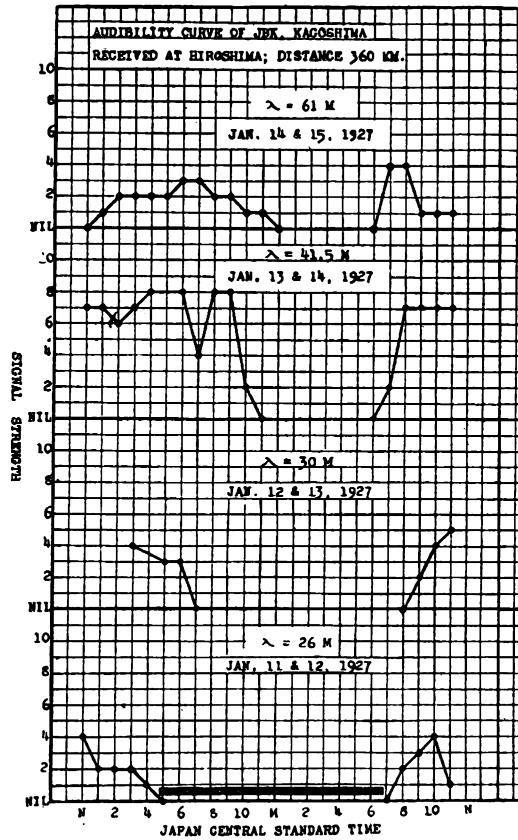

FIG 1 - B

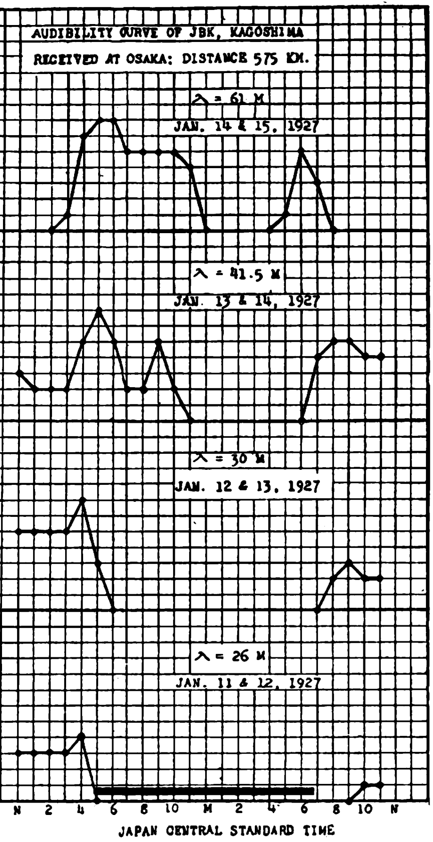

FIG. 1 - C

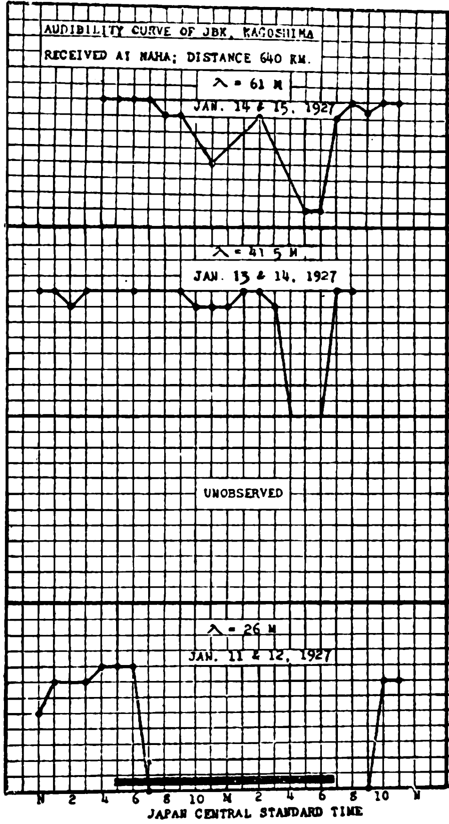

FIG. 1 - D

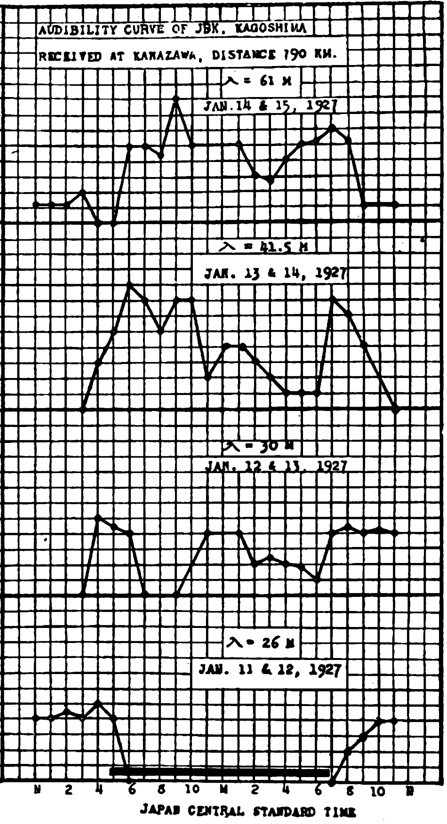

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FIG. 1 - E

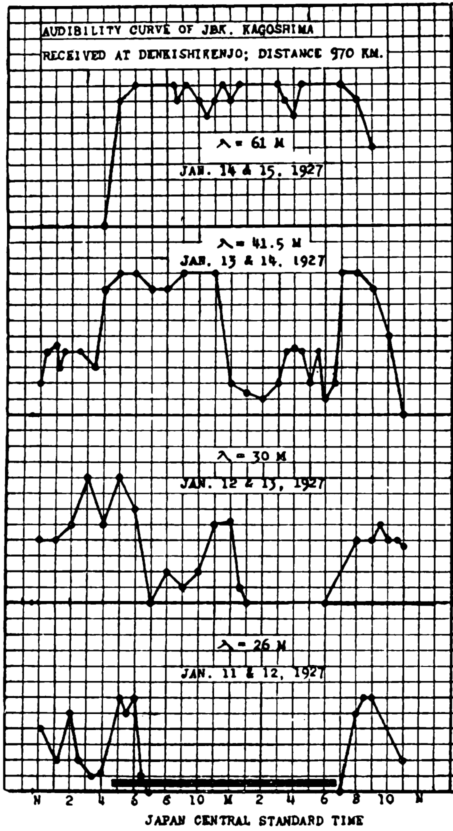


FIG. 1 - F

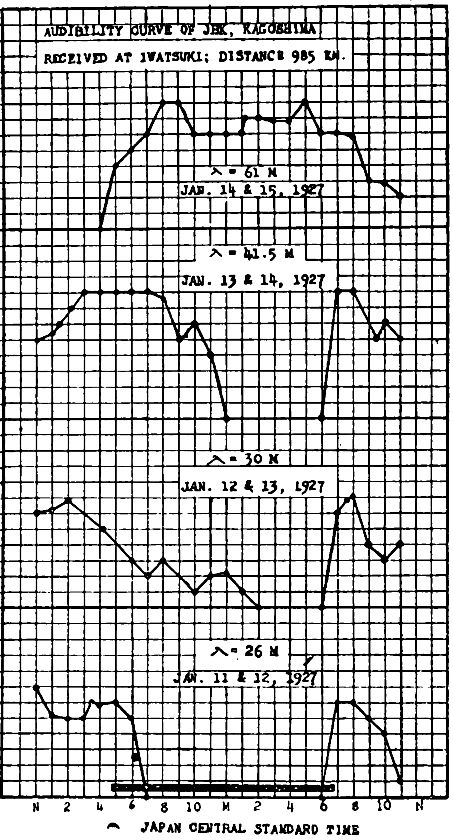


FIG. 1 - G

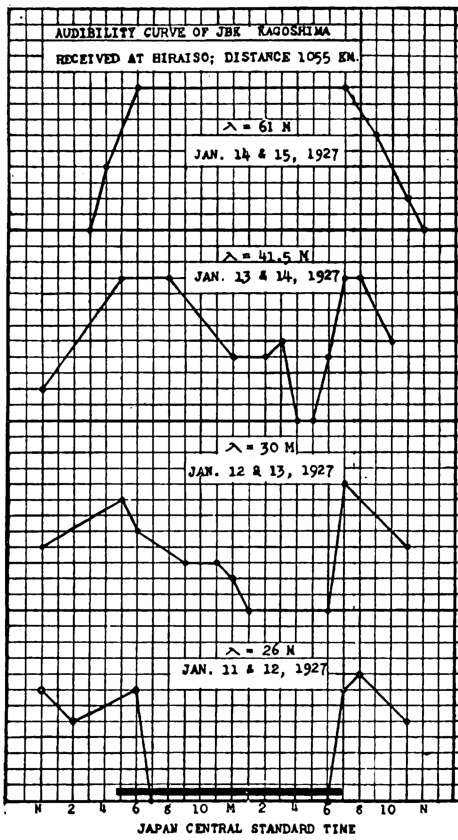
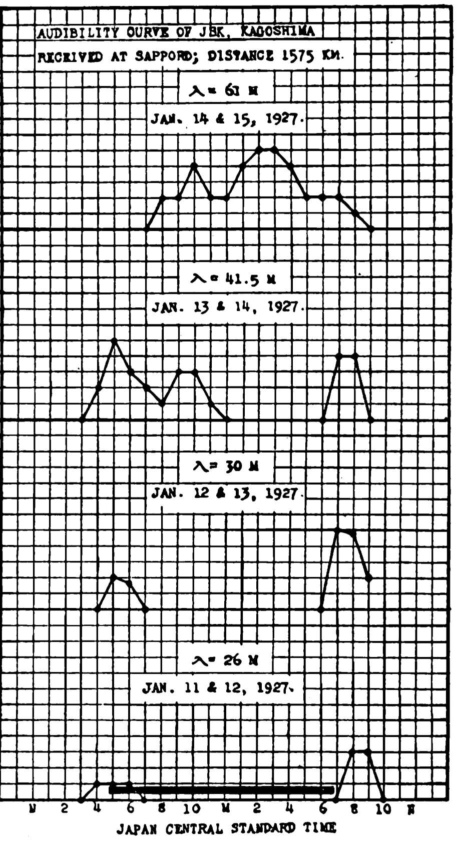


FIG. 1 — H



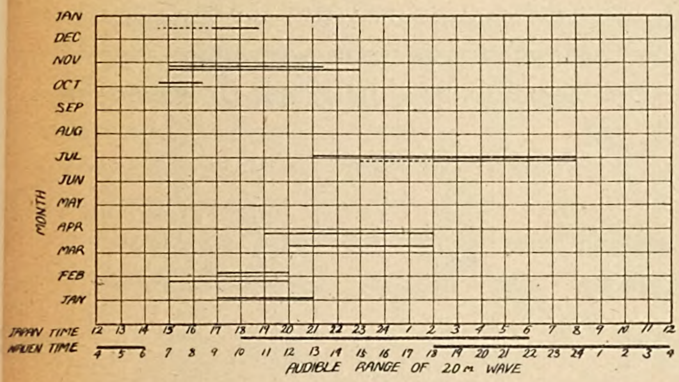
Seasonal Variation in Signal Strengths of the 20 meter Wave from Nauen in Japan,

by T. NAKAGAMI and T. ONO,

Direction General of Telegraph and Telephone Engineering,
Ministry of Communications, Tokyo (Japan).

We made a series of the receiving tests of the 20 meter waves from Nauen at Iwatsuki Receiving Station near Tokyo for more than one year, and got the result which is summarized in the annexed figure. The hours during which Nauen signals were readable are shown for several seasons, in which we tried to receive the Nauen 20 meter wave for 24 hours.

In summer, there are about 11 hours of signal readability when the path from Nauen to Japan supposed to lie on the great circle is night-time, but in winter, they are reduced to only a few hours, shifting towards the time when the great circle passing through Nauen to Japan is daylight time. The great circle between Japan and Germany runs nearly tangential to the arctic circle on its middle part, so that the night-time in summer is not completely dark and the day-time in winter is not completely broad daylight, both resembling the condition of twilight. On this account the degree of ionization of the upper atmosphere is nearly the same in winter day-time and summer night-time; thus the conditions of the propagation of signals seem to be controlled by nearly the same factors.



* * *

The relation between radio reception, sunspot position and area,

by GREENLEAF W. PICKARD,

Wireless Specialty Apparatus C°, Boston (Mass., U. S. A.)

As a matter of years or even months, the relation between radio reception and solar activity is now quite definitely known (1). An increase of solar disturbance, as indicated by an increase in the number of sunspots, solar constant or ultraviolet radiation, is accompanied by an improvement in day reception and a lowering of night reception. By taking periodic daily averages for a number of solar rotations certain general day to day relations can be found; for example, the meridian transit of a sunspot group is usually accompanied by a depression of night reception and (with the exception of 1926 and the early part of 1927) a slight increase of day field (2).

Night reception in that portion of the broadcast band between 750 and 1,500 kilocycles and over East-West transmission paths shows the highest relation to measures of solar activity. But even with this most sensitive type of radio transmission, if intervals of less than a month are taken the degree of correlation rapidly decreases Comparing night reception at Newton Centre, Massachusetts, from stations WBBM and WGN at Chicago, with Wolfer Provisional Sunspot Numbers over the interval January, 1926 to September, 1927, the following correlations are found :

INTERVAL	CORRELATION	r/e
—	—	—
Three months . .	— 0,92 ± 0,038	24,0
Two months . . .	— 0,75 ± 0,093	8,1
One month . . .	— 0,54 ± 0,10	5,4
Fifteen days . . .	— 0,45 ± 0,084	5,4
Seven days . . .	— 0,30 ± 0,06	5

When a day to day comparison of reception and Wolfer sunspot numbers is attempted, a very low correlation is found. If recognition is taken of the distance of the sunspots from the

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(1) The Correlation of Radio Reception with Solar Activity and Terrestrial Magnetism. (*Proceedings of the Institute of Radio Engineers,* vol. 15, n° 2; February 1927.)

(2) The Correlation of Radio Reception with Solar Activity and Terrestrial Magnetism. *II.* (*Bulletin National Research Council,* n° 61, July 1927, *Proceedings of the Institute of Radio Engineers,* vol. 15, n° 9, September 1927.)

Sun's center, by multiplying their area by (1 — ρ), where ρ is the distance from the center of the solar disc, a somewhat larger correlation coefficient results. A further improvement results when only spots within a few degrees of the central meridian are taken. But even when position as well as area is considered, the connection between individual sunspot groups and reception is not sufficient to show that we are dealing with cause and effect.

As a working hypothesis, I have assumed that there are definite solar areas which are responsible for reception disturbances, and which usually persist for a number of solar rotations. In general these active areas are also areas of sunspot formation, but they are effective in producing reception disturbances even when they contain no spots. Large sunspot groups may also occur entirely outside these active areas, in which event the earthward presentation of such spots is less likely to be accompanied by changes in reception.

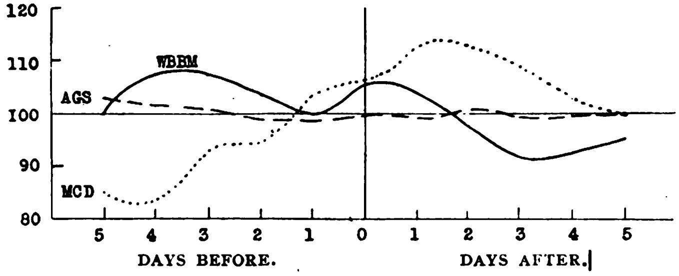

FIG. 1. — Reception and magnetism with maxima of 4 or more spots in central 13.3° band. 88 cases, Februari 17, 1926 to May 21, 1927.

But as the active areas persist for a number of solar rotations (sometimes over a year) and are the most likely places for sunspot formations, we may still use sunspot area and position to derive general relations despite their inadaquacy in the individual case. It is the object of this paper to show that the central passage of sunspots does not exactly coincide with the associated reception disturbances, and that the interval between the transit of the sunspots and the reception change varies both with the area of the sunspot group and with the particular period taken for investigation.

In the first seven figures night reception, day reception and terrestrial magnetism are compared with the central passage of sunspot maxima. The night reception is taken from my measurements at Newton Centre, Massachusetts, and Seabrook Beach, New Hampshire, of station WBBM at Chicago, the day reception is at Washington from AGS at Nauen, Germany, while the magnetic measure used is the Magnetic Character of Day Number given by the Observatory at Cheltenham, Maryland. The sunspots were counted in a central band 13,3° wide, which is one day's travel in solar rotation.

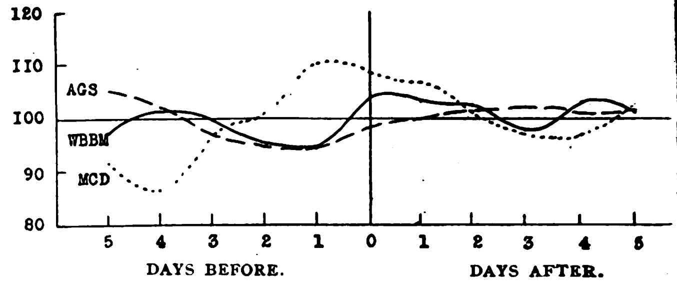

FIG. 2. — Reception and magnetism with maxima of 4 to 10 spots in central 13.3° band, 62 cases, February 17, 1926 to May 21, 1927.

In figure 1 is shown the relation between day and night reception, terrestrial magnetism and sunspot maxima of four or more spots, over the period February 17, 1927, to May 21, 1927. The ordinates represent percentage variation of the elements, and the graph covers an interval of eleven days. Night reception, shown in full-line, reaches a minimum about three days after the central passage of the spots, day reception an ill-defined minimum one day before, while magnetism, shown in dotted line, shows a maximum of disturbance between two and three days after.

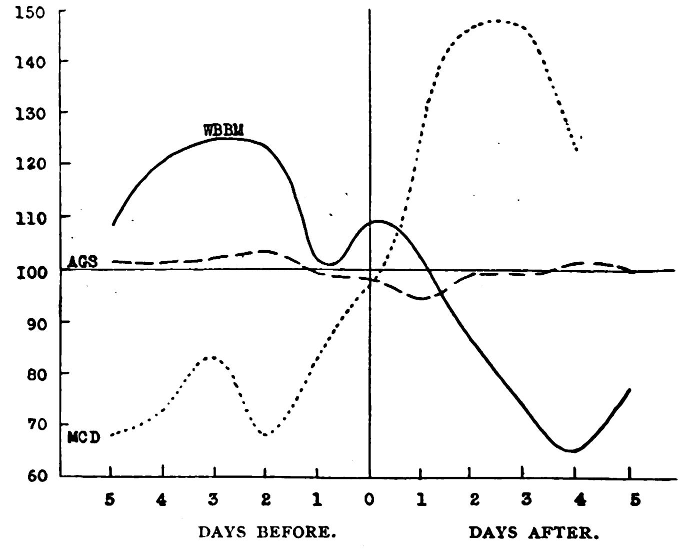

FIG. 3. — Reception and magnetism with maxima of 12 or more spots in central 13.3° band. 26 cases, May 13, 1926 to May 12, 1927.

A comparison of reception and magnetism over the same period, but with sunspot maxima limited to four to ten spots is shown in figure 2. Now both night and day reception show minima one or two days before, with a maximum of magnetic disturbance one day before.

In figure 3 only the larger sunspot groups are taken, with spot numbers of twelve or more in the central band. The amplitude of the night reception and magnetic disturbance has now increased, with minimum and maximum values, respectively, four and three days after the passage of the spots, while day reception shows a slight depression one day after.

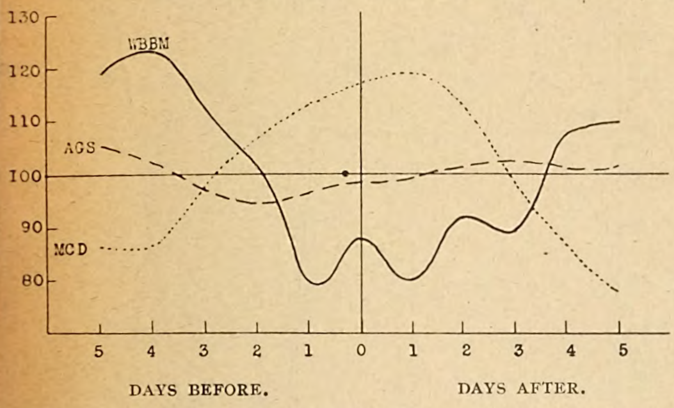

FIG. 4. — Reception and magnetism with maxima of 4 to 10 spots in central 13.3° band. 26 cases, February 17, 1926 to October 4, 1926.

It is evident from these figures that the interval between sunspot transit and reception depression is related to sunspot area, much as if the smaller spot groups were on the Western, and the larger spots on the Eastern boundary of the active areas. But there is another factor to be considered, namely the period covered by the observations. In the preceeding figures this has been approximately fifteen months; now this interval will be broken into two nearly equal parts. In figure 4 the interval is from February 17, 1926 to October 4, 1926, with sunspot maxima of four to ten. Night reception shows a depression centering one day after, magnetism a maximum one day after, and day reception a minimum two days before.

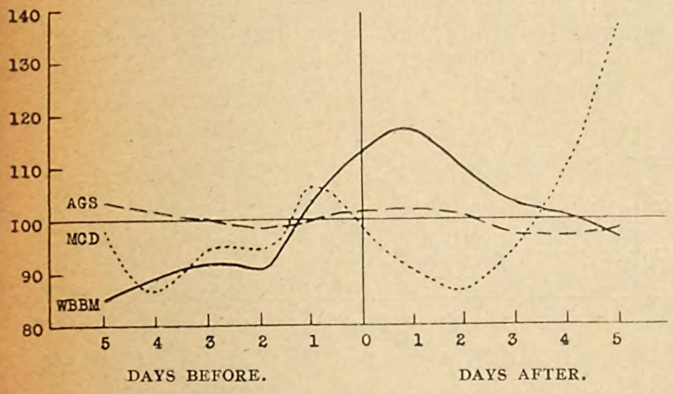

FIG. 5. — Reception and magnetism with maxima of 4 to 10 spots in central 13.3° band. 36 cases, October 8, 1926 to May 21, 1927.

In figure 5 the period is October 8, 1926 to May 21, 1927, with sunspot maxima of four to ten. Although at first sight this figure is the inverse of the preceeding one, the night reception maximum one day after the passage of the spots is not related to this particular transit, but merely represents reception recovery from some preceeding disturbance; actually what has happened is merely a considerable displacement in time. The inverse relation of magnetism and night reception is here the best guide to the true position of the reception minimum, which is more than five days after the spot transit. Apparently the smaller sunspot groups were quite near the center of the active areas during the first seven months, but in the second period they moved East by at least 65°.

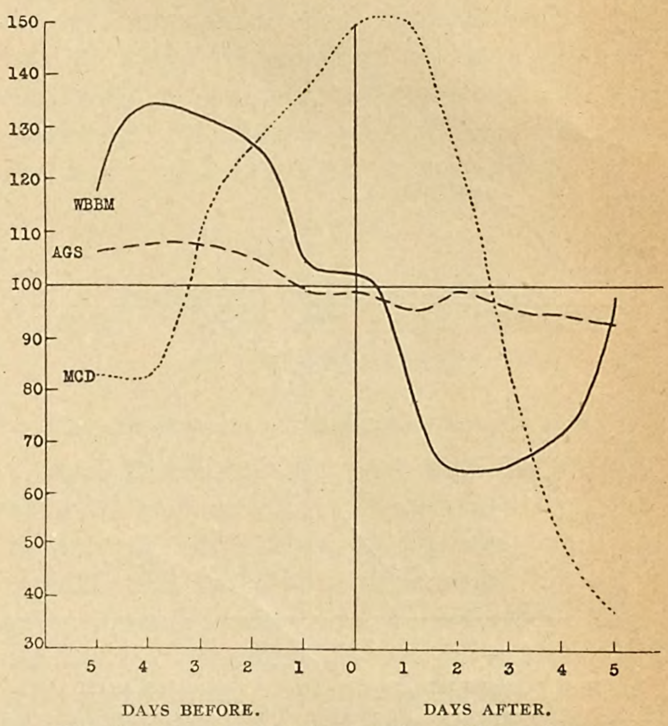

Fig. 6. — Reception and magnetism with maxima of 12 or more spots in central 13.3° band. 8 cases, May 13, 1926 to September 19, 1926.

The larger sunspot groups shows a similar displacement, altho to a lesser degree. In figure 6 reception and magnetism from May 13, 1926 to September 19, 1926, are compared with sunspot maxima of twelve or more. Night reception shows a minimum two days after, and magnetism a maximum one day after. Figure 7 is also for sunspot maxima of twelve or more, for the period of October 11, 1926 to May 12, 1927, and here the reception and magnetic elements show a minimum and maximum values three and four days, respectively, after the transit of the spots; a displacement of about two days to the right.

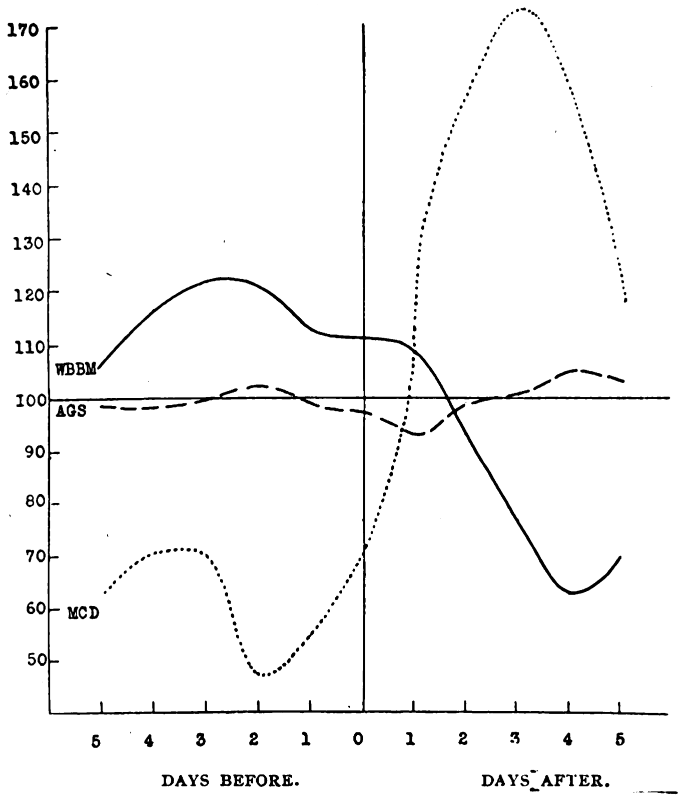

FIG. 7. — Reception and magnetism with maxima of 12 or more spots in central 13.3° band. 18 cases, October 11, 1926 to May 12, 1927.

It is aparent from the preceeding figures that the relation of reception to magnetism is much more intimate than its connection with sunspots. In figure 8 the magnetic storm is taken as the event, instead of sunspot transit. Within the nineteen months from January, 1926 to August, 1927, there were fourteen magnetic storms recorded by Cheltenham, and the depression of night reception accompanying these storms is very striking. Day reception shows an inverse effect, an increase of field accompanying and following the storm. The night reception maximum three days before is not in anticipation of the coming storm; it merely represents the recovery of reception from the depression associated with a preceeding magnetic disturbance.

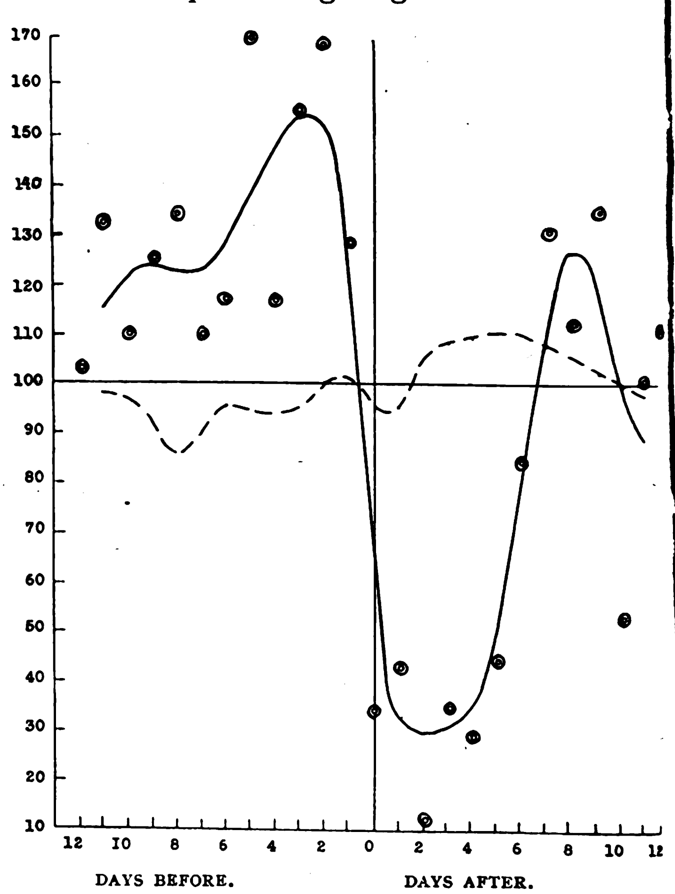

FIG. 8. — Reception from WBBM and WGN with magnetic storms observed at Cheltenham. 14 cases, January 26, 1926 to August 19, 1927. — AGS day reception in broken line.

Altho the most marked reception disturbances accompany magnetic storms, even a moderately disturbed day has its associated reception depression. In figure 9, which covers the period January 24 to June 12, 1927, all

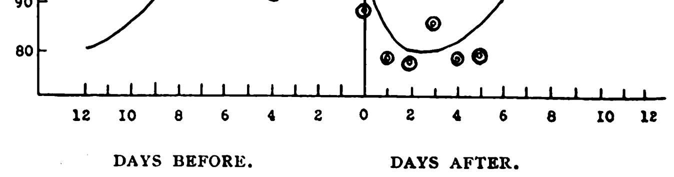

FIG. 9. — Mean reception from WBBM and WJAX at Washington and Newton Centre with maxima of magnetic disturbance at Cheltenham. 36 cases, January 24 to June 12, 1927.

magnetically disturbed days are taken, that is, all days with character number of one or two. The night reception data are here an arithmetic average of WBBM at Washington and Newton Centre, and WJAX of Jacksonville, Florida, at Washington. Over the first half of 1927 magnetic disturbances came at intervals of approximately fifteen days, so the reception maximum three days before represents the recovery of reception from the depression associated with a preceeding magnetic disturbance, and the curve has a period of fifteen days.

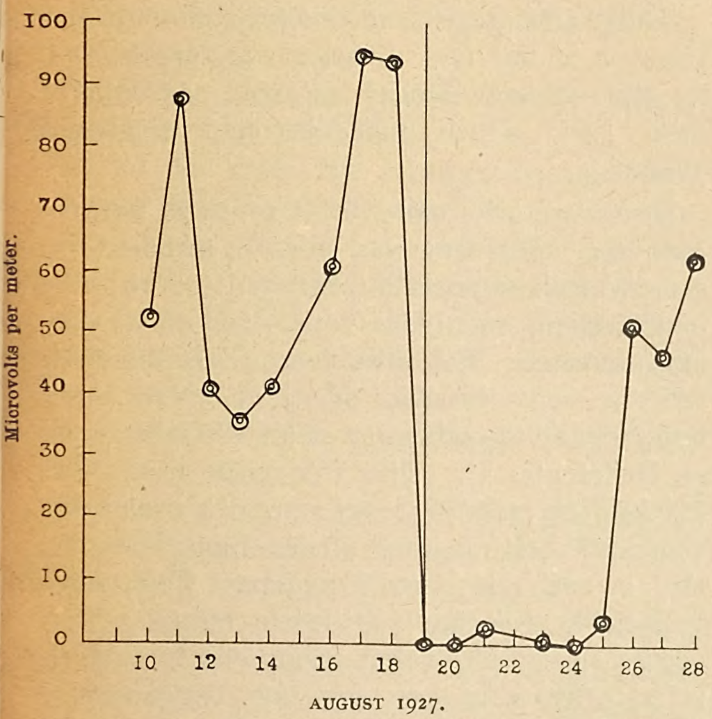

FIG. 10. — Night reception from WGN, Chicago, at Seabrook Beach, N. H., and the magnetic storm of August 19 and 20, 1927.

On the 19th and 20th of August, 1927, there was a magnetic storm of considerable intensity, with displays of Northern lights on both evenings, visible thruout New England. In figure 10 I have shown the reception depression associated with this storm, in which the field from WGN remained at a low value for a week.

Night reception over any extended period is largely a series of abrupt depressions followed by relatively slow recoveries. The recovery, at least during a sunspot maximum, is rarely completed before the next depression occurs, so that the mean field for, say, a month, is largely determined by the interval between these abrupt depressions. If this interval is short, for exemple a week, the mean field will be low; if two or three quiescent weeks intervene the mean field will be high.

These depressions quite definitely accompany disturbances of terrestrial magnetism, and less definitely the central passage of sunspot groups, but in neither case are we warranted in assuming that the connection is one of cause and effect. However, the relation of reception to terrestrial magnetism is so definite it is difficult to escape the conclusion that disturbances of these two elements arise from a common and nearby cause; an electrical change in our atmosphere. Any inexactness in this relation might be explained on the assumption that magnetic disturbances require an electrical movement, whereas reception is dependent upon the degree and disposition of ionisation.

* * *

Apparent night variations with crossed coil radio beacons,

by HARADEN PRATT,
Radio Engineer,
Bureau of Standards, Washington D. C. (U. S. A.)

It early became apparent that the employment of coil antennas on airplanes for determining the directions or bearings of radio stations was beset with difficulties. The coils were limited in size and considerable amplification was needed to secure a signal of suitable strength. Airplane engine ignition interferences in the radio receiving system prevented the use of very much amplification and so the distance range was limited. Further, the high level of noise present on aircraft prevented a close observation of the minimum signal sector during rotation of the coil, thereby by impairing the accuracy of that method as applied to aeronautical navigation.

Various means for the taking of radio station bearings from airplanes were attempted without realizing a practical solution of this problem and it was not until the crossed coil radio beacon was proposed that those interested in the art became encouraged to expect the evolution of a simple and workable system to guide aircraft.

The crossed coil beacon idea was investigated by the Bureau of Standards several years ago, since which time its development has been rapid, and there are now several of them available for service in the eastern part of the United States.

Essentially, this beacon comprises two coil antennas disposed in two vertical planes fixed at an angle with each other. This two coil system is free to be rotated about a vertical axis. When the coils are similarly excited at radio frequency, signals of equal intensity from each will be heard on a receiving set when located along any one of the four vertical planes bisecting the angles between the planes of the coils. At other points the signal intensities from each coil will be different. This marking off in space of a vertical equisignal plane constitutes the directive feature of the crossed coil type of beacon, and this equisignal zone is frequently referred to as the course set down by it.

In its practical application and for convenience in construction, the beacon used in this country employs a radiating system of two large fixed single-turn loops. A suitable goniometer interposed between these loops and the power source permits by its rotation, the rotation in space of the equisignal zones. A mechanical device automatically transmits the letter A on one coil and the letter N on the other. These are so interlocked that a continuous long dash is heard along the equisignal zone. The width of this zone where the dash only is heard depends largely upon the acuteness of the observer's attention, and may vary between limits, for example, from 1 1/2 to 3 degrees.

For the guidance of aircraft the crossed coil beacon possesses several obvious advantages.

1. There is no zone of minimum or maximum signal strength to be observed.

2. Location of the course or beam is secured by an automatic comparison of two signals.

3. Regardless of the position of the aircraft within a wide angle when off the course, this beacon furnishes a definite signal enabling the craft to locate, and return to its proper course. This characteristic permits temporary detours to be made during flight around stormy areas or obstructions, a very important and valuable feature.

4. The aircraft uses an ordinary receiving set with the usual trailing wire antenna permitting signals to be received under the most favorable conditions as regards minimum sensitivity of airplane receiving set to local noise and disturbances.

5. An aircraft may be guided along a set and invariable airway regardless of wind drift.

These advantages had much to do with the selection of this type of beacon for application to our national airway system, particularly those over which schedule mail airplanes operate.

To secure information of a practical nature, receiving equipment was recently installed on a mail airplane operating between the airports of Cleveland and New York, over an airway approximately 380 miles long. Two directive beacons were available, one at the New York terminal and the other at a point 170 miles west at Bellefonte, Pa. The frequency used was 290 kc. The radiating loops were of a triangular shape 300 feet long and 80 feet high. The current in each loop was 8 amperes. This route crosses the Alleghany mountain ranges which occupy nearly all of that region and is a particulary favorable one for this experimental flying both because of the rough topography and the prevalent foggy and cloudy weather.

The accuracy of the directive beacon as a guide had already been established as of a high order, through considerable use in the past, but upon examination of the situation it was learned that all previous experience with it had been confined to daylight flying. Its operation at night had been untried. Flights over the airway mentioned were therefore undertaken last summer and results inconsistent with those secured by day were immediately observed. During the first flight it seemed impossible to keep the airplane on a course corresponding to the interlocking dash signal. No sooner was this signal received when it would change to the letter A or letter N. No amount of manipulation of the airplane would improve the situation. The dash signal would come and go at intervals of a few minutes. Its soon became apparent that at this distance which was about ninety miles, no accurate fixed course existed but that the equisignal zone was moving about in an indefinite way.

This disconcerting effect indicated that a study of the phenomenon must be made, and several night flights up to distances of 175 miles from the beacon were undertaken. In every case the shifting of the zone or course was noticed. The general results secured from five flights made at an average altitude of 2,000 feet may be stated as follows :

1. Within 25 miles of the beacon the shifting was not of a serious nature.
2. At 50 miles the shifting became pronounced but due to the zone being stationary in its proper position for possibly 75 °/₀ of the time, the beacon could still be depended upon.
3. At a distance of 100 miles the shifting became very pronounced and persisted for more than 50 °/₀ of the time.
4. At 125 miles the beacon was of no further value as a guide.
5. The shifting of the zone was gradual so that at first one would be inclined to think it due to the movement of the airplane.
6. It appeared that the topography of the country between the beacon and the airplane exerted a considerable influence on the extent of the variation.
7. Exceptional variations in shift over as much as 100 degrees in azimuth were noted, but in general the change was confined to within possibly 25 degrees.
8. Beyond 15 miles the fading of the general level of signal received was very severe during flight over mountains. Several variables being involved, no conclusions have been reached as to the relative contribution of each factor, except that more steady signals were observed at fixed points on the ground than in the air, at similar distances from the beacon.

A few observations of the Bellefonte beacon at night have been made on the ground at Washington. While present, the shifting phenomenon was less pronounced than that observed in the air. On a night flight from Harrisburg to Washington no shifting of the zone was noticed, using signals from a beacon at College Park, Md. As there are no marked mountain ranges near College Park, these observations would indicate that topographical features have an important bearing on the matter of these variations.

To shed further light on the question, two sets of night-time measurements on the Bellefonte beacon have recently been made, one at a point 22 miles and another 32 miles distant, both locations being in mountainous territory. Records obtained with a graphic field intensity recorder were made of signals using both a vertical antenna and a coil antenna placed in a vertical plane extending towards the beacon. Ratios of the variability to average field intensity were observed to be,

at 22 miles with	coil antenna . . .	0.25
»	vertical antenna. .	0.00
at 32 miles with	coil antenna . . .	0.44
»	vertical antenna. .	0.07

Ratios of the variability to the maximum field intensity were observed to be,

at 22 miles with	coil antenna . . .	0.43
»	vertical antenna. .	0.05
at 32 miles with	coil antenna . . .	0.56
»	vertical antenna. .	0.1

Rotating the coil antenna around its vertical axis so as to receive minimum signal showed a variation in the direction of the arriving field as large as 30° over a ten minute interval.

In closing I might mention that although the equisignal zone ordinarily remains stationary in its correct position during daylight hours, sudden shifts of a few moments duration have been noticed in the mountainous country, particularly when flying down river valleys at altitudes of 1,000 to 1,500 feet.

It is hoped that this brief outline of these few preliminary observations made by the staff of the Bureau of Standards, will serve to focus attention on the performance of this type of radio beacon so that its limitations as a guide for flights at night may be studied and practical information secured whereby these performance characteristics and the conditions influencing them may become better understood.

October 12, 1927.

* * *

A Radio Frequency Oscillator for Receiver Investigations,

by George RODWIN and Theodore A. SMITH,
Technical and Test Department, Radio Corporation of America
(New York, N. Y., U. S. A.)

A modulated radio frequency oscillator forms a piece of apparatus which is extremely useful in an experimental laboratory. By incorporating a number of special features, such an oscillator can be made very flexible and its uses varied.

The equipement to be described was originally built in connection with work done in determining certain radio receiver characteristics (1). The oscillator has also been used for providing known fields in the measurement of field intensities, determining coil resistance and inductance, lining up radio circuits, and for other general laboratory purposes.

The design was such as to satisfy as far as possible the following requirements :

1. The oscillator output should be widely variable without any appreciable change in frequency.
2. A means for reading the output should be provided.
3. The frequency should be variable over a given range (such as the broadcast band) and the calibration should be maintained.
4. At any frequency setting, it should be possible to vary the frequency in small continuous steps to either side of the setting with a fair degree of accuracy.
5. Modulation should be obtainable up to at least 50 °/₀ with means for indicating continuously the percentage of modulation.
6. The stray field from the apparatus should be low compared to the field produced by the known radiator used.
7. Mechanical details should be such as to enable the operator to take data quickly and easily.

The apparatus to be described meets these requirements satisfactorily. Many details have been thoroughly developed and retained in this device, with the result that the oscillator may appear rather large and bulky. However, when it is considered to what varied use it may be put, its large size is justified.

ELECTRICAL CONSTRUCTION.

The complete apparatus consists of a modulated oscillator-power amplifier arrangement, with an output and measuring circuit. A schematic diagram appears in Fig. 1.

General Circuits. — It was found that the master-oscillator-power amplifier method for the generation of radio frequency currents had several advantages over the use of a simple oscillator. The oscillator employs a UX — 201A tube in an ordinary Hartley circuit. A grid condenser of 500 micro-microfarads with a grid leak of 20,000 ohms gives sufficient constancy of frequency for small variations in filament and plate voltage, without greatly reducing the radio frequency output. For the broadcast band, an inductance of 82 microhenries with a variable condenser of 1,000 micro-microfarads maximum capacity forms the tuned circuit. The variable condenser, which is of the straight line frequency type, is purposely made large, so that only the really straight portion of its frequency calibration is normally used, while still maintaining a three-to-one range in frequency. In order to minimize the effect of tube capacity in different tubes on the frequency calibration, a small fixed condenser of about 60 micro-microfarads capacitance is placed in parallel with the main tuning condenser. This addition was found to improve the frequency-setting relation of the main control. Figure 2 shows this relation for the apparatus constructed.

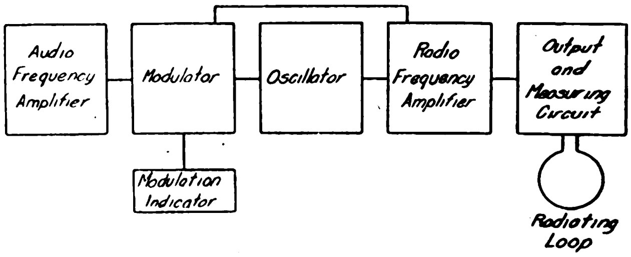
Audio Frequency Amplifier
Modulator
Oscillator
Radio Frequency Amplifier
Output and Measuring Circuit
Modulation Indicator
Radiating Loop

Fig. 1. — Schematic Diagram of Modulated Oscillator-Amplifier.

A mechanical arrangement is employed for varying the frequency in small steps. This device is explained in detail below. Frequencies in steps of 1,000 cycles up to 8,000 cycles to either side of a given frequency can be obtained. The calibration of this adjustment was carried out as follows :

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(1) *Proceedings of the Institute of Radio Engineers*, vol. XV, n° 5, May 1927, p. 389.

With the vernier control at its zero position, a zero beat is produced in a radio receiver with a separate external oscillator, the frequency of which is known to be constant over the period of the calibration. When the vernier is moved off its zero position, a beat note will occur, which is compared with a calibrated audio frequency source, adjusting the pitch of the beat-note so that it is equal in frequency to the known audio frequency. This operation was repeated for a number of frequencies up to 8,000 cycles to either side of the vernier zero position.

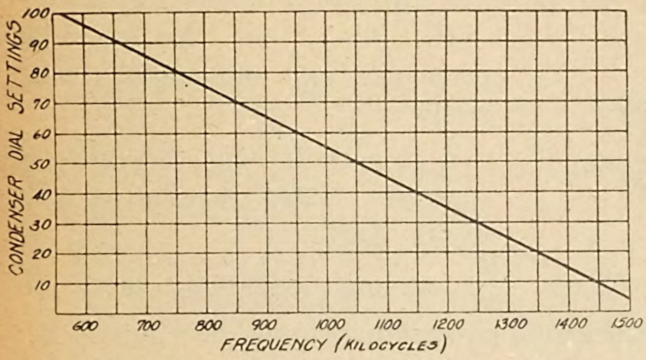

FIG. 7. — Apparatus Frequency. — Setting Relation.

Since the vernier control is constructed so as to move the tuning condenser a constant amount, in angular motion, for a given rotation of the vernier, and since the variable tuning condenser has a straight line frequency characteristic, the calibration of the vernier will hold for all radio frequencies. This was checked by repeating the above beat-note comparisons for various condenser positions and the vernier calibration was found to be practically constant over the whole frequency range.

The radio frequency power amplifier has its grid circuit coupled to the oscillator circuit by means of an untuned inductance. Two UX — 112 tubes are employed in parallel and these are neutralized. The power amplifier is operated as a « proper amplifier » and its grid excitation is such as to obtain maximum output under these conditions. The amplifier plate circuit is tuned by a condenser placed on the main tuning condenser shaft.

The output circuit is coupled to the amplifier plate coil, its coupling being continuously variable for output control. It was found desirable to have quite a large range of output — at least 5,000 to 1 in current. The combination of a radio frequency current transformer, thermo-couple milliammeter and switching arrangement has been found satisfactory for providing and metering such a widely varying output.

Current Transformer for Provision of Various Output Currents. — The current transformer for radio frequencies (1), in the form employed in this apparatus, has an iron core of 10 mil (0.0254 centimeters) toroidal silicon steel laminations, 9/16 inch (1.43 centimeters) thick, 2 inches (5.08 centimeters) outside diameter, and 1 inch (2.54 centimeters) inside diameter. The core is covered with insulating oiled silk and one winding of 100 turns is evenly spaced over this core. The other winding of either 1 or 10 turns is looped through the hole in the toroid. Wood blocks keep the windings well spaced from one another so as to reduce capacity effects between the windings and maintain the current ratio. The construction is shown in Fig. 3.

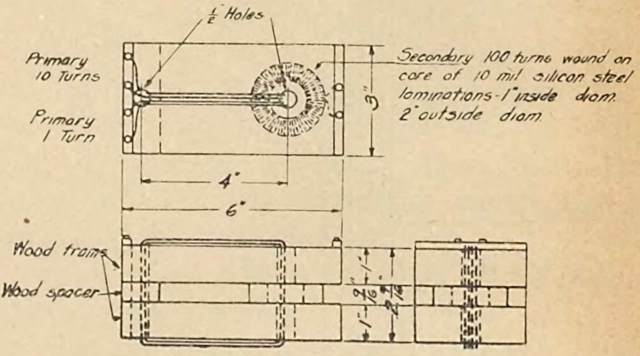
1/2" Holes
Primary 10 Turns
Primary 1 Turn
Secondary 100 turns wound on core of 10 mil silicon steel laminations-1" inside diam. 2" outside diam.
3"
4"
6"
Wood frame
Wood spacer
1"
9/16"
2 9/16"
1"

FIG. 3. — Current Transformer for the Measurement of Output Currents.

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(1) D. W. DYE, *Measurement of Alternating Electric Currents of High Frequency.* (Proceedings of Royal Society [of Great Britain] A-1914, vol. XC, p. 621.)

Bureau of Standards Circular 74, p. 153.

D. W. DYE, *Current Transformer Methods of Producing Small Voltages and Currents at Radio Frequencies for Calibrating Purposes.* (Journal Institution of Electrical Engineers, London, June 1925, vol. LXIII, p. 597.)

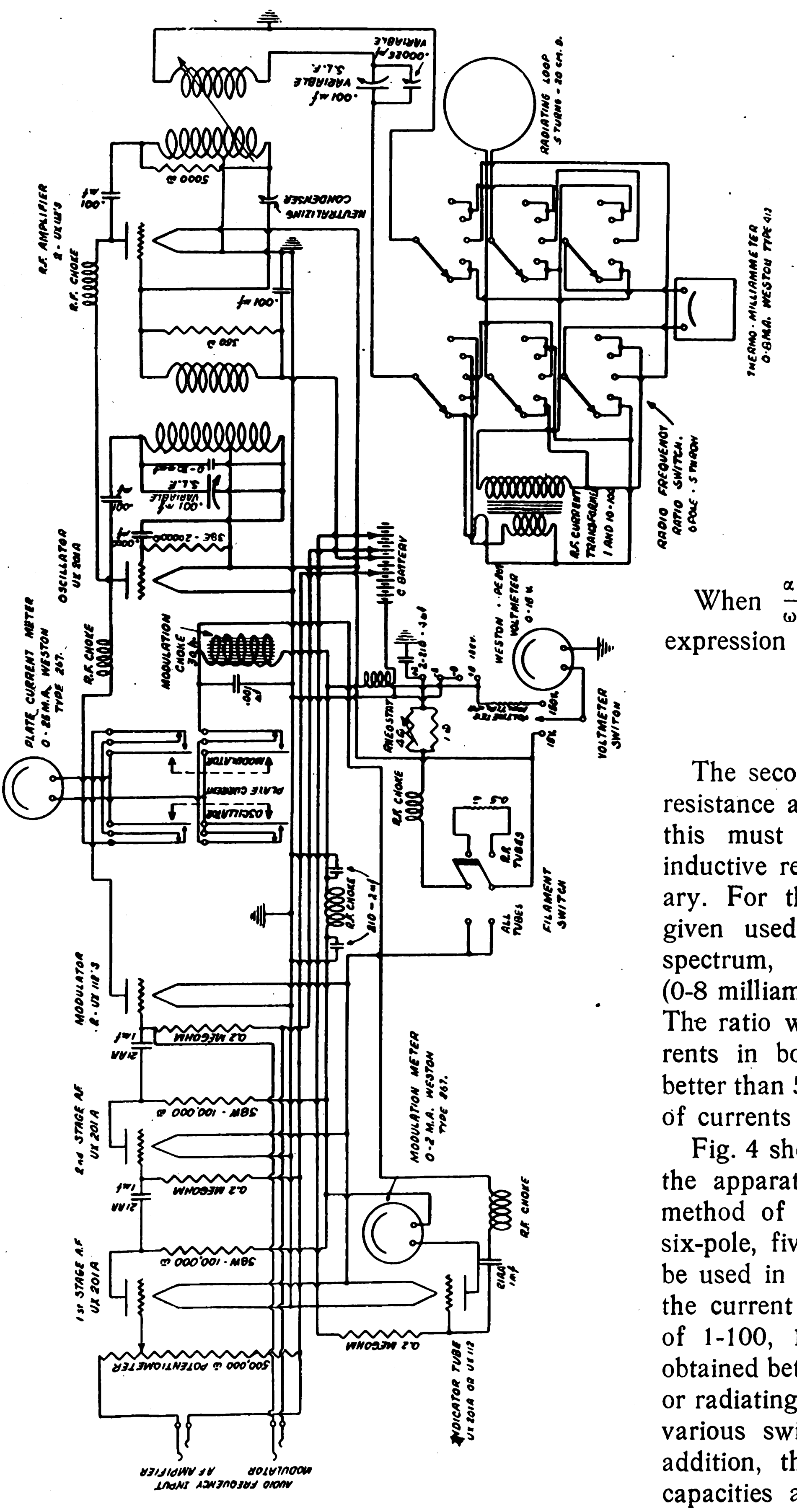
RADIATING LOOP 5 TURNS - 20 CM. D.
NEUTRALIZING CONDENSER
R.F. AMPLIFIER 2-UX's
R.F. CHOKE
THERMO-MILLIAMMETER 0-8 M.A. WESTON TYPE 412
R.F. CURRENT TRANSFORMER 1 AND 10-100
RADIO FREQUENCY RATIO SWITCH 6 POLE - 5 THROW
OSCILLATOR UX 201A
C BATTERY
WESTON - PEAK VOLTMETER 0-15 V.
MODULATION CHOKE
PLATE CURRENT METER 0-25 M.A. WESTON TYPE 267
R.F. CHOKE
VOLTMETER SWITCH
RHEOSTAT
MODULATOR
PLATE CURRENT
OSCILLATOR
R.F. CHOKE
R.F. TUBES
ALL TUBES
FILAMENT SWITCH
MODULATOR 2-UX 112's
MODULATION METER 0-2 M.A. WESTON TYPE 267
0.2 MEGOHM
2nd STAGE AF UX 201A
1st STAGE A.F. UX 201A
R.F. CHOKE
INDICATOR TUBE UX 201A OR UX 112
500,000 Ω POTENTIOMETER
AUDIO FREQUENCY INPUT
MODULATOR
AF AMPLIFIER

Fig. 4.— Complete Circuit Diagram of the Apparatus.

It can be shown that the current ratio for such a transformer is given by an expression of the form

$$\frac{I_1}{I_2} = \frac{n_2}{n_1}\left(1 + \frac{\alpha R_2}{\omega L_2}\right)$$

where I_1 and n_1 are the primary current and turns number respectively.

I_2 and n_2 are the secondary current and turns number respectively.

R_2 and L_2 are the secondary resistance and inductance respectively.

α is a coefficient depending on the iron losses, having a value usually less than 1.

When $\frac{\alpha R_2}{\omega L_2}$ is small compared to 1, this expression reduces to

$$\frac{I_1}{I_2} = \frac{n_2}{n_1}$$

The secondary circuit contains the ammeter resistance and, for accuracy of transformation this must be made low compared to the inductive reactance of the transformer secondary. For the transformer of the dimensions given used at frequencies in the broadcast spectrum, a 43 ohm thermo-milliammeter (0-8 milliamperes) has been found satisfactory. The ratio was checked by measuring the currents in both windings and an accuracy of better than 5°/₀ was found throughout the range of currents and frequencies required.

Fig. 4 shows the complete wiring diagram of the apparatus and from it may be seen the method of using the current transformer. A six-pole, five point switch allows the meter to be used in either the primary or secondary of the current transformer so that current ratios of 1-100, 1-10, 1-1, 10-1, or 100-1 may be obtained between the meter circuit and the load or radiating loop circuit. The capacity between various switch parts has been kept low. In addition, the effects due to any undesirable capacities are minimized by the use of low impedance load and metering circuits.

Provision of Known Fields. — The radiating loop with a known current in it may be used to induce an easily calculated voltage in a second loop having a known mutual inductance between loops. Known field intensities in a plane x centimeters from the loop and parallel to the plane of the loop can be calculated from the field H which is

$$H = \frac{2\pi a^2 in}{10} \times \frac{1}{(x^2 + a^2)^{3/2}} \text{ gausses}$$

The field intensity in $\frac{\text{volts}}{\text{cm}} = 300$ H.

Thus,

$$E = 18850 \frac{a^2\, in}{(x^2 + a^2)^{3/2}} \times 10^6 \text{ microvolts per meter}$$

where a is the radius of the loop in centimeters.

i is the loop current in amperes.

n is the number of turns on the loop.

If the plane in which it is desired to know E is at an angle to the plane of the loop, the field intensity may be calculated from the above expression, introducing a factor of the cosine of the angle between planes. Field intensities ranging from ten to one million microvolts per meter are readily obtained in this manner.

Modulation. — The conventional plate circuit choke coil method is used for modulation, two UX — 112 tubes controlling both master-oscillator and power amplifier. This system has been found satisfactory for the purpose. Fairly high degrees of modulation are obtainable. The relation between input voltage and percentage modulation has been found linear up to 70 °/₀. For measuring the percentage of modulation, the ordinary method of measuring the audio frequency voltage across the modulation reactor and comparing this value with the direct current plate voltage of the oscillator and modulator will not apply. This is due to the fact that both the oscillator and radio frequency amplifier are being modulated. The cathode ray oscillograph has been used for modulation-percentage determination. One set of the oscillograph plates is excited from the voltage induced in a pick-up coil coupled to the radiating loop. The oscillator is modulated from an audio frequency source. The other set of oscillograph plates is supplied with voltage from the same audio source. In general, the resulting oscillograph figure is a trapezoid. The various forms it may take are shown in Fig. 5.

Using the symbols of Fig. 5, the modulation is given by $\frac{2x}{m} \times 100$ °/₀. But $m = \frac{a+b}{2}$ and $x = \frac{a-b}{4}$ therefore modulation $= \frac{a-b}{a+b} \times 100$ °/₀.

As an indicating device, a UX — 201A tube is employed with its normal plate current biased to practically zero and its grid excited from the audio voltage developed across the modulation reactor. The plate circuit of this tube contains a 0-2 milliampere ammeter the scale of which has been calibrated directly in modulation percentages using the oscillograph method described. A radio frequency choke and by-pass condenser prevent the radio frequency voltage from affecting the indicator tube reading. With the radio frequency amplifier tubes operating properly and the circuits equalized, the modulation indication is practically constant with varying radio frequency.

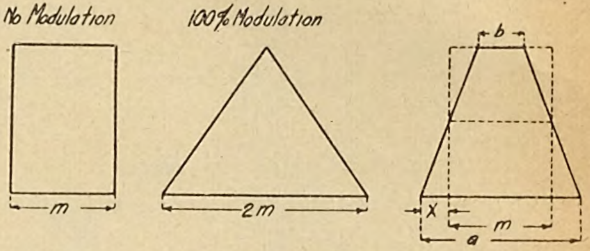

Fig. 5. — Cathode-Ray Oscillograph Figures used in Modulation Percentage Determination.

A two-stage resistance-coupled amplifier supplies sufficient voltage to the modulator grids. Its characteristic is practically flat over a wide range of audio frequencies.

Equalization of Output Current. — The radio frequency output current was found to vary considerably with frequency and a constant output was believed to be desirable. A means for correcting this fault to a considerable extent was devised. This consisted of placing suitable fixed resistances across both the radio frequency amplifier coupling coil and its output coil. The variation of output current in the radiating loop was thus made less than 15 °/₀ for a constant output coupling setting over the frequency range. These resistances were also found to lessen the effect of the tuned circuit on the audio characteristics.

Overall audio frequency characteristics from audio amplifier input to modulated radio frequency output are shown in Fig. 6. The data for these curves was taken by rectifying some of the voltage picked up with a biased detector tube and observing the audio frequency output. A slight effect of resonant circuit selection still remains, but this is not of any considerable magnitude and can be allowed for in exact measurements.

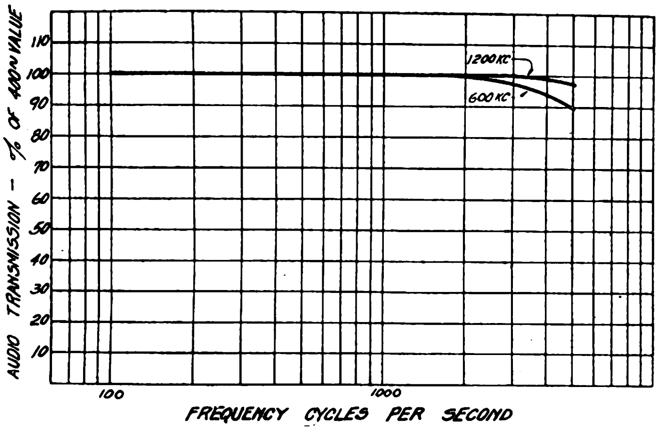

FIG. 6. — Overall Audio Frequency Characteristics of the Apparatus.

MECHANICAL CONSTRUCTION.

Tuning and Vernier Controls. — The oscillator and output tuning condensers are both mounted on the same shaft and are controlled from a single dial on the front panel. A friction cam arrangement has been found suitable for vernier adjustment. This consists of a lever and friction plate on the main shaft and a cam on a separate vernier shaft. The end of the lever arm bears on the cam and is held in position by a strong spring. Any movement of the vernier shaft will cause the lever to move either up or down a small amount and, since the friction plate is now bound to the main shaft, the main tuning condensers will move a small amount. The main tuning shaft can be moved without disturbing the vernier arrangement, since the friction plate will slip and the vernier lever arm will remain fixed due to the combined action of the spring and the cam rest. The criterion of whether such a system will operate is that the force required to move the friction plate over its bearing should be greater than the bearing friction in the tuning condensers. In practice, this is easily accomplished. The details may be seen in figures 7 and 8.

The whole apparatus is mounted in a double copper shielded box. The front panel and base board have been made removable as a unit for convenience. The various units, such as audio frequency amplifier, oscillator, radio frequency amplifier, and output circuit, are shielded from one another. The details may be seen in figures 9 and 10. The grid batteries are located in a separate compartment, but the filament battery of 6 volts and the plate battery of 135 volts are located outside the shielded box. Radio frequency filters in the battery leads prevent radiation from these.

The thermo-milliammeter for the measurement of output is placed externally near the apparatus, a twisted pair of leads of short length being used for connections.

In conclusion, we desire to mention that this design was carried out under the direction of Mr. Julius Weinberger, whose suggestions were very valuable in the development of the equipment.

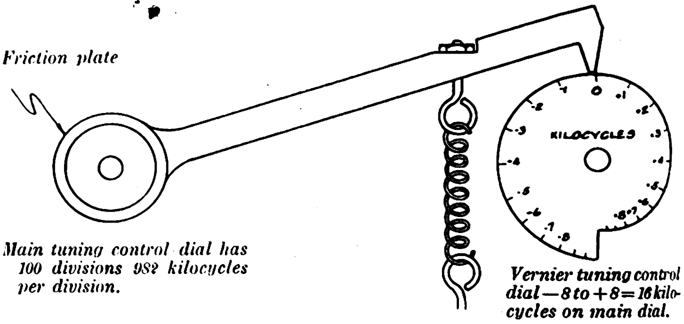
Friction plate
KILOCYCLES
0
.1
.2
.3
.4
.5
.6
.7
.8
Main tuning control dial has 100 divisions 982 kilocycles per division.
Vernier tuning control dial — 8 to + 8 = 16 kilocycles on main dial.

FIG. 7. — Cam and Lever Used for Vernier adjustement.

SUMMARY.

A modulated radio frequency oscillator is described. This apparatus incorporates a means for obtaining radio frequency outputs of widely varying range, a metering system, a means for changing the generated frequency in small steps to either side of a given frequency, and a modulation and indicating system.

* * *

The effect of Retroaction on the Received signal strength (*),

by BALTH. VAN DER POL, D. Sc.
(Eindhoven-Holland)

The considerable increase in signal strength obtainable through the use of retroaction (feed back) is well known. It is also common knowledge that this increase in signal strength is considerably greater when originally the incoming signal is weak than when it is strong. It is the purpose of this paper to provide a non-linear theory of this effect of retroaction and the experimental verification thereof.

Suppose a triode system of one degree of freedom, as in Fig. 1 given, where L, C, r form the tuned circuit and the mutual induction M provides the retroaction.

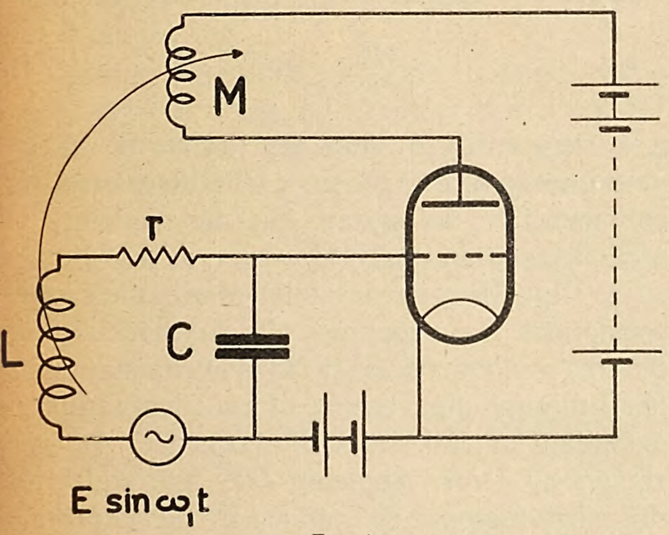

FIG. 1.

Let further an E. M. F. E $\sin\omega_1 t$, representing the « incoming signal » act on the oscillatory system. Neglecting the grid current and calling the current in the LCr circuit i, and the deviation of the anode current from its steady value i_a and the alternating grid P. D. v_g we have

$$L\frac{di}{dt}+ri+\frac{1}{C}\int idt - M\frac{di_a}{dt} = E\sin\omega_1 t, \qquad (1)$$

$$\frac{1}{C}\int idt = v_g. \qquad (2)$$

In the « tuned grid » -circuit, the variations of anode potential are usually small compared with the variations of the grid potential, so that as a first approximation, the anode current variation i_a is a function of the grid potential variation only, i. e.

$$i_a = f(v_g). \qquad (3)$$

In order to avoid complexity we neglect the grid current and we therefore imagine a negative grid bias to be provided. Our system of coordinates in the i_a, v_g plane is therefore as shown in Fig. 2. This differs from the usual notation in so far as the zero point of the coordinate system is shifted towards the steady d. c. position round which the oscillations occur.

Equation (3), representing the curved anode current -grid potential characteristic of the triode, we approximate by the cubic :

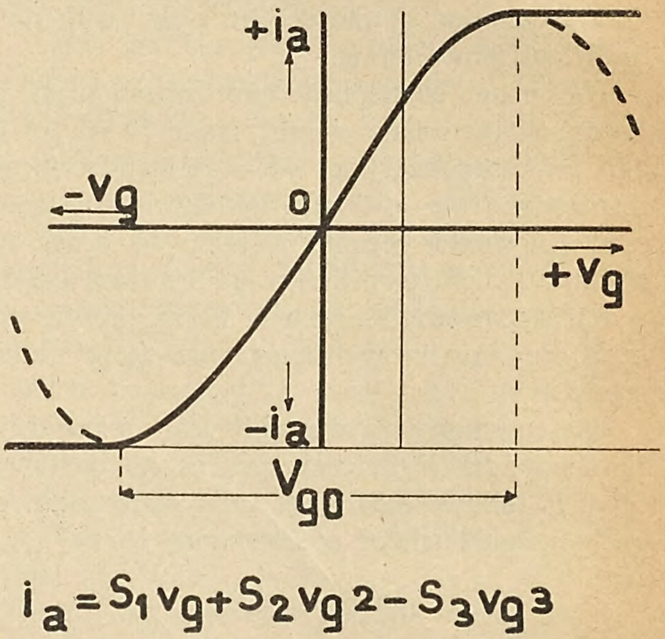

FIG. 2.

$$i_a = S_1 v_g + S_2 v_g^2 - S_3 v_g^3 \qquad (3a)$$

where S_1 is the usual « mutual conductance » for infinitesimal grid potential variations. S_2 and S_3 are further determined by the form of the characteristic. As the latter bends round both at the top and at the bottom we write in (3a) $- S_3 v_g^3$ in stead of $+ S_3 v_g^3$.

The elimination of i and i_a from (1), (2) and (3a) results in :

$$\left.\begin{aligned}\frac{d^2 v_g}{dt^2}+\left\{\left(\frac{r}{L}-\frac{MS_1}{LC}\right)-\frac{2MS_2}{LC}v_g+\frac{3MS_3}{LC}v_g^2\right\}\frac{dv_g}{dt}\\ +\omega_0^2 v_g = \omega_0^2 E\sin\omega_1 t.\end{aligned}\right\} \qquad (4)$$

(*) Paper read at the Meeting of the U. R. S. I. at Washington D. C., October 1927.

where

$$\omega_0^2 = \frac{1}{LC}.$$

Calling further

$$\frac{r}{L} - \frac{MS_1}{LC} = -\alpha,$$

$$\frac{MS_2}{LC} = \beta,$$

$$\frac{MS_3}{LC} = \gamma,$$

(4) becomes

$$\ddot{v}_g + (-\alpha - 2\beta v_g + 3\gamma v_g^2)\dot{v}_g + \omega_0^2 v_g = \omega_0^2 E \sin \omega_1 t, \quad (4a)$$

which is a non linear inhomogenous differential equation of the second order with non linear resistance terms.

The usual elementary approximation of a linear characteristic would make $\beta = \gamma = 0$ and for resonance (i. e. $\omega_0^2 = \omega_1^2$) and critical retroaction (i. e. $\alpha = 0$) the developing grid p. d. v_g would become infinite. In order to obtain a satisfactory theory of the response of a retroactive triode system to an impressed E. M. F. a non linear problem must therefore be solved.

The equation (4*a*) was fully considered in a former paper (*); from the results obtained there it follows that the steady state solution in the neighbourhood of resonance

$$(\mid \omega_0 - \omega_1 \mid \ll \omega_1)$$

can be written :

$$v_g = b \sin (\omega_1 t + \varphi),$$

where the amplitude *b* of the resulting grid potential variation is given by :

$$b^2 \left\{ 4(\omega_0 - \omega_1)^2 + \left(\alpha - \frac{3}{4}\gamma b^2\right)^2 \right\} = \omega_0^2 E^2, \quad (5)$$

a cubic equation in b^2. It is further seen from (5) that the asymmetrical term in (4*a*) (with β) (which determines the detection) has, in the first approximation here considered, no influence on the resulting v_g. *Therefore as a first approximation detection has no effect on the h. f. grid potential difference developping under the influence of an incoming « signal ».*

Further, if (5) is compared with the usual linear case as represented by

$$b^2 \{ 4(\omega_0 - \omega_1)^2 + \alpha_0^2 \} = \omega_0^2 E^2$$

it follows that in the non-linear case $(\alpha - \frac{3}{4}\gamma b^2)$ is substituted for α_0, i. e., the system behaves as if for the resistance

$$r$$

a new resistance r' were substituted of a value given by

$$r' = r - \frac{M}{C}\left(S_1 - \frac{3}{4} b^2 S_3\right), \quad (6)$$

which therefore depends upon the amplitude *b* present already in the system (*).

When the retroaction is pushed so far that

$$\frac{MS_1}{C} > r, \quad (7)$$

it is seen from (6) that the first order differential resistance of the system becomes negative and therefore the system has the tendency to oscillate spontaneously. However, as was shown in the Phil. Mag. paper quoted above, the forced oscillations may suppress the development of the free oscillations which phenomena manifests itself through the presence of a « silent region » extending at both sides of resonance. As was shown by Prof. Appleton (**) the width of this silent region is determined by the amplitude of the incoming signal. For strong signals this width is given by

$$\frac{\omega_0 - \omega_1}{\omega_0} = \pm E . \sqrt{\frac{3\gamma}{\alpha}}. \quad (8)$$

It is of interest to investigate the resultant grid amplitude *b* if

a, the system is tuned exactly to resonance, i. e.

$$\omega_0 = \omega_1,$$

and **b**, it is brought on the verge of free oscillation, i. e. when

$$S_1 M = Cr. \quad (9)$$

(*) Phil. Mag., **3**, 65, 1927.

(*) This property of the non-linear system was first derived in 1920, see : Balth. Van der Pol, *Radio Review*, November, December 1920.

(**) E. V. Appleton, *Proc. Camb. Phil. Soc.*, **23**, 231, 1923.

For this case we at once obtain from (5)

$$\frac{3}{4}\gamma b^3 = \pm \omega_0 E, \qquad (10)$$

which expression will now be considered in detail.

Firstly it follows from (10) that *the resulting grid amplitude is proportional to the cube root of the E. M. F. acting on the system.*

Further it is easy to assign an approximate value to γ directly from the triode characteristic.

Referring to (3*a*), (4*a*) and Fig. 2, and taking the symmetrical case for which $S_2 = 0$, it follows that a good approximation to S_3 can be found from the maximum and minimum value of the cubic (3*a*), for $i_a = i_{a\,\max.}$ when $v_g^2 = \frac{S_1}{3S_3}$.

Calling therefore the grid voltage change necessary to bring the anode current from zero to its saturation value the « grid space » and designing it by V_{g0} (see Fig. 2), we obtain

$$V_{g0}^2 = \frac{4S_1}{3S_3},$$

hence

$$\gamma = \frac{M}{LC}\,\frac{4S_1}{3V_{g0}^2}.$$

But, when the system has critical retroaction (9) obtains, i. e.

$$M = \frac{Cr}{S_1},$$

hence

$$\gamma = \frac{r}{L}\cdot\frac{4}{3V_{g0}^2}.$$

When we further call v_{g1} the grid voltage amplitude which would be obtained with no retroaction at all ($M = 0$) or (which is the same thing) with dimmed filament, we find from (5)

$$v_{g1}^2 = \frac{\omega_0^2 L^2}{r^2}\cdot E^2,$$

hence we obtain from (10) :

$$b^3 = v_{g1}\cdot V_{g0}^2$$

or

$$\boxed{b = \sqrt[3]{v_{g1}\cdot V_{g0}^2}} \qquad (12)$$

reading in words :

The grid amplitude developping in resonance and with critical retroaction equals the cube root of the product of the grid amplitude which would be obtained with no retroaction at all, and the square of the « grid space » V_{g0}, as defined by Fig. 2.

(12) can further be written :

$$\frac{b}{v_{g1}} = \left(\frac{V_{g0}}{v_{g1}}\right)^{2/3} \qquad (13)$$

which means that : *the amplification obtained through retroaction equals the two third power of the ratio of the « grid space »* V_{g0} *into the amplitude obtained with zero retroaction.* The much greater gain obtained through retroaction with small signals than with stronger ones is at once apparent by (13).

Some measurements provided a very satisfactory experimental verification of the theory outlined above.

The measurements were taken with relatively low frequency, thus avoiding obvious errors. That the results are however equally applicable to high frequency circuits follows at once from the following theorem :

If a model is made of a high frequency system consisting of linear inductances, linear capacities and non linear resistances (e. g. triodes) and if the values of all the inductances (self and mutual) and capacities in the model are made n *times these values in the original high frequency system, but if the resistances (linear and non-linear) in the model are made equal to the resistances in the original circuit, the currents and potentials occurring in this model will be equal in magnitude to the currents and potentials in the original high frequency system but, considered as a function of the time, they will vary* n *times slower* (*).

Therefore the natural periods of the model will be *n* times those of the original system, and the building up and decay of currents in the model will also occur *n* times slower.

This simple theorem at once follows from a

(*) Incidentally in this model system the stray capacities are reduced *n* times in magnitude. Therefore, in order to investigate the effect of a specified stray capacity in the original high frequency circuit it is only necessary to insert at its place in the model circuit a capacity *n* times the original stray capacity.

dimensional consideration of the coefficients such as

$$r, \quad \omega L, \quad \frac{1}{C\omega}, \quad M\omega,$$

occurring in the differential equations, because e. g.

$$L\omega = Ln \cdot \frac{\omega}{n}, \text{ etc.}$$

In fact, with a system having a natural frequency of 500 Hertz and with critically adjusted retroaction it often took a minute for the free oscillation of the triode system to reach its final value, hence it was not easy to decide, before the external E. M. F. was applied, whether the retroaction was exactly critical or not. Therefore an intermediate way was chosen and a model of a receiving set was made having a natural frequency of 15,000 Hertz.

Four sets of readings were taken (*) with a standard triode (tungsten filament $V_f = 4.0$ volt, saturation current 11 mA., anode potential varying between 100 and 200 volts, negative grid bias varying between — 4.5 and — 7.5 volt).

The applied E. M. F. $E \sin \omega_1 t$ was varied between 2.10^{-6} and 10^{-2} Volts, the resulting alternating grid voltage v_{g_1} with no retroaction varied between 100.10^{-6} and 0.5 Volts, and the resulting alternating grid voltage b with critical retroaction varied between ca. 0.2 and 4 Volts. The exponent S in the formula

$$b = (V_{g0}^2 \cdot v_{g1})^S$$

which, according to the above theory, should be

$$S = 0.33$$

was found from the four sets of measurements to be

$$\begin{aligned} S = {} & 0.36 \\ & 0.36 \\ & 0.36 \\ & 0.32 \\ \hline \text{Mean : } S = {} & 0.35 \end{aligned}$$

while the « grid space » V_{g0} calculated from these measurements was

$$\begin{aligned} V_{g0} = {} & 26 \text{ Volts} \\ & 25 \\ & 20 \\ & 15 \\ \hline \text{Mean : } V_{g0} = {} & 22 \text{ Volts,} \end{aligned}$$

as compared with the value

$$V_{g0} = 27 \text{ volts}$$

obtained directly from the characteristic.

Especially the experimental value for the exponent S fits in well with the theoretical value; provided only those parts of the characteristic are considered for which no oscillation hysteresis occurs. The representation of the i_a, v_g characteristic, by a cubical parabola, with three constants S_1, S_2 and S_3 only, obviously cannot yield very accurate values for V_{g0} obtained e. g. with the aid of a voltage swing of not more than 0.35 Volts.

Therefore, there is no doubt that for all practical purposes the theory expounded above fits in well with experiment.

Finally we give some practical figures calculated from formula (12) with the following data :

$$\frac{\omega L}{r} = 40,$$

$$V_{g0} = 27 \text{ Volts.}$$

Electromotive force E working in grid circuit.	Resulting alternating grid voltage with no retroaction (v_{g1}).	Resulting alt. grid volt. b, with critical retroaction.	Amplification obtainable through critical retroaction (b/v_{g1}).
10^{-6} Volts.	$0{,}04.10^{-3}$ Volts.	0,31 Volts.	7,700
10^{-5} »	$0{,}4\ .10^{-3}$ »	0,66 »	1,600
10^{-4} »	$4{,}0\ .10^{-3}$ »	1,4 »	360
10^{-3} »	0,04 »	3,1 »	77
10^{-2} »	0,4 »	6,6 »	16

Physical Laboratory
of Philips' Glowlampworks Ltd.
Eindhoven (Holland).

* * *

(*) The experiments and calibration of the necessary amplifiers were performed, by MM. K. POSTHUMUS and R. VELDHUYZEN. Care was taken that the « receiver » did not react on the « transmitter ».

The navy's primary frequency standard (1),

by Robert H. WORRALL and Raymond B. OWENS,
Naval Research Laboratory,
Washington (D. C., U. S. A.)

Synopsis. — In this paper is described a constant frequency source which is used as the Navy's primary working standard of frequency. This frequency source is a crystal controlled oscillator of special design which was developed at the United States Naval Research Laboratory, Washington, D. C. This oscillator employs a type of circuit which is rich in harmonics, over 200 of which may be used as fixed standard frequencies.

It also describes in detail the method employed to determine the fundamental frequency of this standard in terms of Naval Observatory time to an accuracy of about one part in 100,000. The ultimate standard of frequency is therefore the mean solar day.

History. — Several years ago when the Navy began to experiment with high frequency continuous wave radio communication, the Navy standard of frequency was an absorption type of wavemeter. It consisted of a precision condenser and a number of coils designed to have a constant inductance and a low radio-frequency resistance. A resonance indicator, if desired, could be loosely coupled to this circuit. This standard wavemeter was calibrated by means of the multi-vibrator or other oscillator whose output was rich in harmonics. The multi-vibrator in turn was adjusted to resonance with a standard 1,000 cycle tuning fork. By this means the wavemeter calibration could be maintained by frequent checking to an accuracy of one-tenth of one percent. For spark and low frequency continuous-wave transmission, precision of this order was satisfactory.

One of the first results of the advent of high frequency transmission was to indicate the inadequacy of a frequency standard the calibration of which might be in error by 0.1 °/o in either direction. For at a frequency of 10,000 kilocycles, this would mean a possible error of 10 kilocycles, plus or minus. Thus at this frequency a transmitter would occupy a band 20 kilocycles wide, which would increase interference between stations, or greatly decrease the number of communication channels by requiring a greater separation in kilocycles between assigned frequencies. Naturally a service wavemeter calibrated from this standard might conceivably be in error by a much larger percent than the standard itself.

Therefore there was presented to the Naval Research Laboratory for solution a three-fold problem : 1° to develop for the Naval service a frequency standard whose constancy should be as nearly absolute as possible; 2° to provide a means of obtaining direct from this standard a large number of fixed frequencies for calibration purposes; 3° to develop a method for determining the frequency of this standard in terms of observatory time to better than 0.01 °/o, and as near 0.001 °/o as possible.

Crystal controlled oscillators. — The application of the piezo electric properties of quartz crystals to control the frequency of vacuum tube oscillating circuits, regardless of small changes in capacity or inductance, suggested a crystal controlled oscillator as the solution to our first problem. After experimenting with different types of circuits and making a number of refinements in crystal holders a crystal controlled oscillator was developed which with automatic temperature control was found to give an output of practically absolute constancy.

Tests have shown that the combined effect on the frequency of the standard oscillator caused by a five percent change in plate voltage is less than 0.001 °/o. A ten percent change in filament voltage produces no noticeable change in frequency. Changing from one tube to another of the same type produces a change in frequency varying from zero to a maximum of about 0.002 °/o. On account of the lack of uniformity in tube constants, several tubes that give the same frequency as the oscillator tube used in the calibration are kept as spares, which practically eliminates the error due to tubes. The temperature coefficient of the standard crystal in its holder is less than one part in one hundred thousand per degree centigrade.

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(1) Read by Dr. L. P. Wheeler.

Types of circuits. — One type of circuit used in the preliminary work was that shown in Fig. 1. This tuned plate circuit (1), using a small inductance and a relatively large capacity, gives maximum output on the fundamental frequency and a rapidly diminishing amount of energy on the harmonics. In other words, this is a transmitter type of circuit, and only a few harmonics could be used for calibration purposes. This circuit was therefore modified with the purpose of reducing the strength of the fundamental and intensifying the harmonics, so that each multiple of the crystal frequency could be used as a standard frequency. This modification consisted in replacing the tuned plate circuit, with a very large inductance having a small distributed capacity and no condenser across the coil. (See Fig. 2.) This untuned coil must have a natural frequency higher than the crystal fundamental; that is, it must be an inductive reactance at the crystal frequency to give the feed-back in the right phase relation to the grid potential to sustain oscillation.

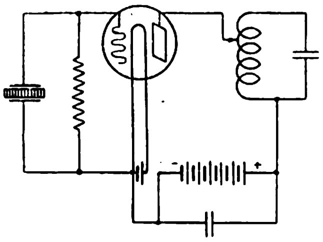

FIG. 1.

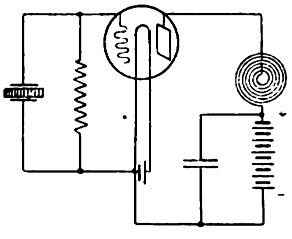

FIG. 2.

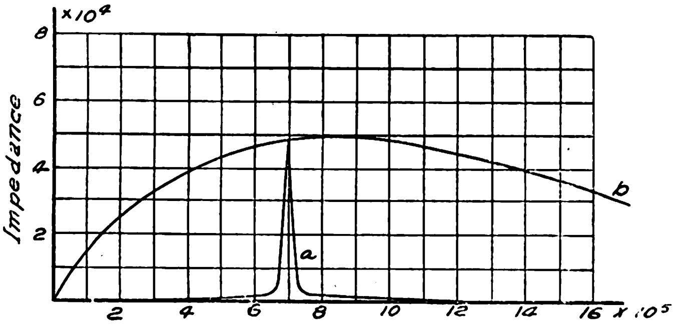

FIG. 3.

To bring out more clearly the difference in the strength of harmonics in these two types of plate circuits, it may be stated that the amount of radio frequency current in the plate circuit of an oscillator on a given harmonic frequency depends greatly on the ratio of the external impedance at this frequency to the internal plate impedance of the tube. When these impedances are about equal for a given frequency, we get the maximum energy on this frequency. If the external circuit impedance is small compared to the tube impedance at a given frequency the energy radiated at this frequency will be correspondingly small.

Now the tuned circuit with a low L/C ratio gives an external impedance of about the order of the tube plate impedance, or greater, over a narrow band of frequencies. (See Fig. 3*a*.) For instance, the external impedance is relatively very small at a frequency five times the crystal fundamental. Therefore this harmonic would be very weak in comparison with the energy radiated on the fundamental frequency.

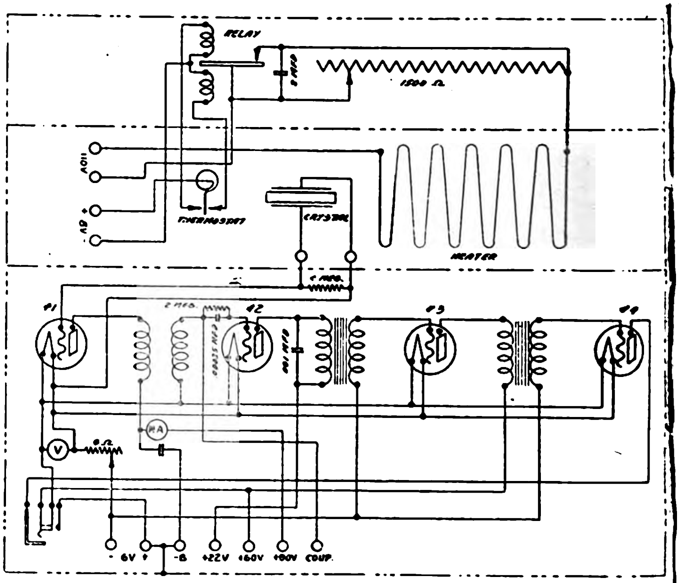

FIG. 4.

On the other hand, a coil with a large L/C ratio gives an impedance curve that is very broad, indicating that the strength of the fundamental and of a large number of successive harmonics will be of the same general order, as indicated in Fig. 3*b*.

This oscillator may be considered as a very low power crystal controlled transmitter radiating on perhaps two hundred frequencies each one of which is an integral multiple of the crystal fundamental frequency.

To make use of all these harmonics, as in the calibration of heterodyne frequency meters, it is necessary to couple this oscillator, as well as any oscillator to be calibrated from it, to a detector and amplifier. The crystal oscillator and the detector and amplifier are built into a single unit, which with the automatic temperature control device is called the *Crystal controlled standard oscillator*. (See Fig. 4.) The input of the detector is untuned and the coil is designed to respond to all frequencies without any adjustments. In practice, the coupling is fixed between the crystal oscillator circuit and the detector coil, and a coupling wire is run from the detector sufficiently close to the oscillator under calibration for coupling purposes. When the heterodyne frequency meter (or other type of continuously variable oscillator) is adjusted to within an audible frequency of any harmonic of the crystal oscillator, the beat note between these is rectified by the detector and amplified by one or two stages of amplification.

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(1) Developed by Dr. J. M. Miller.

The decrease in beat note strength between the successive harmonics of the crystal fundamental frequency and another oscillator is too slight to notice. The signal produced by the hundreth harmonic is of the same general order of audibility as that caused by the fiftieth, and the two hundredth harmonic gives a note of easily readable strength. This device is portable as illustrated in Figs. 5 and 6. At the Naval Research Laboratory the frequency of the standard crystal is 25 kcs. From this one crystal and the associated circuits, as just described standard frequencies are obtained every 25 kcs up to 5,000 kcs. For a high frequency range, another standard is provided with a crystal of a frequency of 250 kcs. These two frequency standards enable us to cover the entire radio frequency scale.

CALIBRATING THE STANDARD OSCILLATOR.

General description of the method used. — Now that a constant frequency source had been developed, capable of giving approximately two hundred points for calibration purposes, the final problem was to provide a means of determining the frequency of this standard to as near 0.001 °/₀ as possible.

The method that was finally decided upon lends itself particularly well to the calibration of low frequency sources, such as the 25 kc standard, but it is not limited to any such frequency. In brief, the method employed makes use of a motor-driven generator whose frequency is varied until some harmonic of the generator output gives « zero beat » with the fundamental of the crystal oscillator. Then the generator is operated at this exact frequency for some minutes during which time a record consisting of a dot per second is imprinted upon a sheet of paper on a revolving chronograph drum thru the medium of a striker actuated by an electrical circuit operated by a standard chronometer. From the slope of the lines of dots thus made on the chronograph drum, the frequency of the generator is very accurately determined. This frequency, multiplied by the number of the harmonic of the generator which was adjusted to the « zero beat » with the crystal oscillator, gives the frequency of the oscillator itself.

To make clear how the method works out in practice, a more detailed description of the apparatus and procedure follows :

Apparatus used in the calibration of the Crystal controlled standard oscillator.

1) A 500 cycle motor generator.
2) A very accurate chronometer.
3) A chronograph.
4) A Maxwell Bridge Circuit.
5) A circuit for the suppression of the 500 cycle fundamental of the generator and the accentuation of the harmonics of the 500 cycle fundamental.
6) A special amplifier.

1) The motor generator outfit consists of a five horse power D. C. motor connected by a heavy fly wheel to an alternator type 500 cycle generator, operated on a high capacity 110 volt battery. (See Fig. 7.)

2) The chronometer is a large Navy standard chronometer adjusted and periodically inspected at the Naval Observatory. The chronometer is checked daily at the Laboratory against the standard time signals. The original rate as

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determined at the observatory was one and one-half seconds a day. Our observations covering several years show a slight seasonal variation in the rate, it being about one second a day in summer and one and one-half seconds in the winter. A correction chart has been plotted showing the rate for any time of the year. But when calibrations of the standard crystal are being made, the rate is determined from the daily check against the time signals.

3) The chronograph consists of the following units; the chronometer, a relay, a striker and a drum. The drum, Fig. 8, is a hollow brass cylinder about 7″ in diameter and 16″ long, accurately machined and geared to the generator by means of reduction gears, giving one rotation of the drum to 150 revolutions of the generator shaft. On this cylinder is placed a piece of paper upon which the striker referred to above makes a record as it moves along the face of the drum thru the medium of a worm. The striker records a dot upon the paper once a second (omitting the 59th second every minute) and is actuated by the chronometer. One rotation of the generator shaft corresponds to an alternating potential output of twenty cycles, as can be noted by counting the poles. Now when the drum, operated through the reduction gear just mentioned, rotates once in exactly six seconds, as indicated by six rows of dots exactly parallel to the axis of the drum on the record sheet, the generator frequency is $\frac{150 \times 20}{6}$ or 500 cycles per second.

If the rows of dots slope up the generator output is less than 500 cycles; if they slope down, it is more. From this chronograph record the exact generator frequency during a run is determined, as will be explained in detail further on.

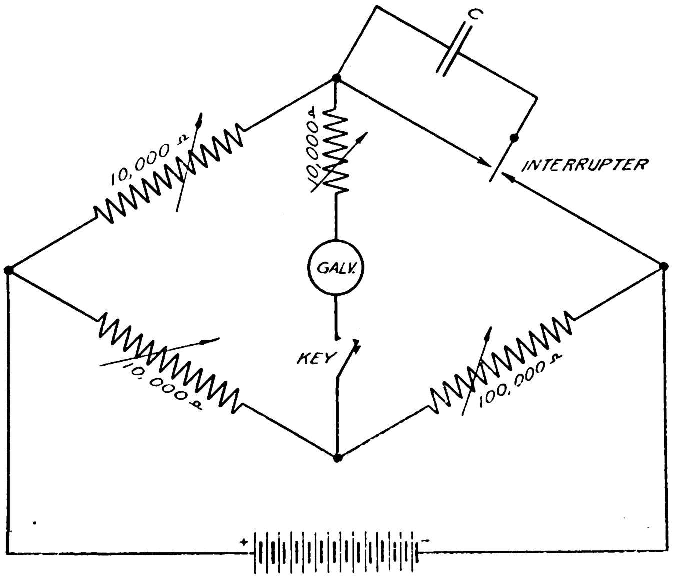

Fig. 9. – Maxwell Bridge Circuit.

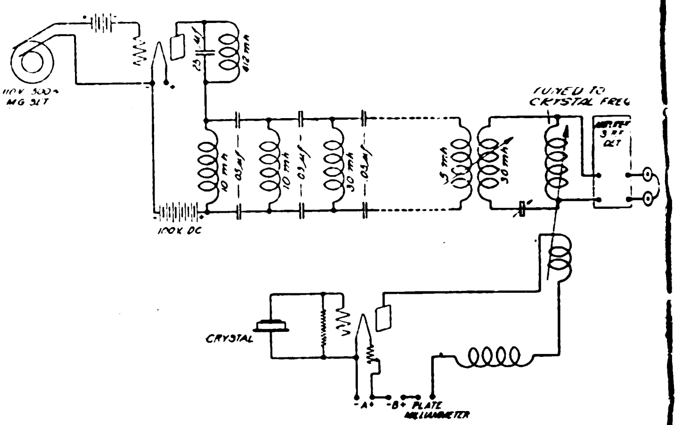

Fig. 10. – Crystal Calibrating Circuit.

4) The Maxwell Bridge, a diagram of which is shown in Fig. 9 is used in connection with the calibration of the standard crystal as an aid in maintaining the speed of the motor generator constant at the value desired.

The condenser shown in one arm, is charged and discharged from the battery across the bridge by means of a rotating interrupter driven from the generator shaft. The commutator segments of the interrupter operate thru three brushes to charge and discharge the condenser.

When the bridge is balanced at a given generator speed, the slightest variation from this speed causes a deflection of the beam of light reflected from the galvanometer mirror on a ground glass scale. The direction of the movement of the light beam indicates whether the generator is running too slow or too fast, thus enabling the operator to vary the pressue on the fly wheel to restore the generator speed to the zero beat condition.

This use of the generator, chronograph and Maxwell Bridge was patterned after the apparatus used at the Bureau of Standards (1) for the accurate measurements of small capacities.

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(1) *Bul. Bureau of Standards*, vol. III, № 4.

5) The method of obtaining harmonics from the output of approximately 500 cycles is to impress this frequency upon the grid circuit of a vacuum tube, the plate circuit of which is rich in the required harmonics. As can be seen from Fig. 10 a tuned resonant circuit in the plate circuit partially absorbs the 500 cycle fundamental which, of course, is very strong The rest of the circuit consists of a filter designed to pass only the high frequency harmonics of the generator fundamental. This filter circuit terminates in a coupling coil which is coupled to a circuit tuned to the crystal frequency. This tuned circuit is also coupled to an amplifier by means of which the beats between the crystal and the generator harmonics are amplified and transmitted to the phones.

6) The amplifier consists of 3 radio stages and a detector.

Making a run. — The frequency of the standard crystal is first determined approximately by means of a standard Wave Meter. This is accurate to about 0.1 of 1 °/₀.

A sheet of paper is placed on the drum and the generator started up. After the generator has warmed up for about 30 minutes the phones from the amplifier shown in Fig. 10 are plugged into the generator room. The operator, with one hand on the Motor Generator flywheel varies the generator speed slightly from 500 cycles until some multiple or harmonic of it approaches the crystal frequency when beats are heard in the telephones. The operator now adjusts the speed until slow beats are heard. With the assistance of the other operator the Maxwell Bridge is balanced, which brings the light beam to rest on the scale. This is done while the first operator is controlling the slight changes in generator speed by changing the pressure upon the flywheel.

The only function of the Bridge is to indicate to the operator by the direction of the deflection of the beam of light whether fast beats in the phones represent an increase or a decrease in generator speed, so that he may quickly increase or decrease pressure on the flywheel to restore the generator speed to its proper value as indicated by very slow beats in the phones. The note in the phones is of the pitch of the generator fundamental frequency which fact may be made clear as follows : when any harmonic of the generator frequency is at zero beat with the crystal, the harmonics next above and below this one give a beat note with the crystal. If the harmonic at 25,000 cycles is tuned to the crystal, then the one at 24,500 and at 25,500 give a 500 cycle note with the crystal. Pulses or beats in this note indicate how much the 25,000 cycle harmonic differs from the crystal frequency. If a few beats are heard on the fast side, the generator speed is reduced just enough to give the same number of beats on the slow side of the zero beat position. A careful operator may thus compensate for slight changes in beat frequency during a run.

The operator now controls the generator speed to give as slow a beat frequency as possible for a few minutes before starting to record the run. When he notes that the machine is stable enough, he closes the chronometer circuit and the striker commences to put a record upon the paper mounted on the revolving drum. The time required to make a run varies from 8 to 15 minutes.

After the run the second operator may make a duplicate run upon the same harmonic, and then both operators make runs on another harmonic. No error due to the human element is observable in the results.

Obtaining the generator frequency. — Before removing the paper record from the drum a line is drawn on it parallel to the axis of the drum. The completed record will contain 6 parallel rows of dots since the drum makes one complete revolution in approximately six seconds, depending upon the frequency of the crystal. The striker puts a dot on the paper every second, while the carriage upon which the striker rests advances approximately 1 millimeter per revolution of the drum. One section of a completed record is shown in Fig. 11.

The method for computing the generator frequency is based on the fact that due to the number of poles in the generator and the ratio of the reduction gear between the generator and the chronograph drum as previously mentioned, a rotation of the drum in exactly six seconds corresponds to a generator frequency of exactly 500 cycles. That is. if the six parallel rows of dots printed on the drum thru the chronometer are parallel to the axis of the drum the generator is operating at 500 cycles.

If the slope of these rows of dots is up, the drum has not made a complete rotation in six seconds and therefore the generator speed is less than 500 cycles. A downward slope indicates more than one rotation of the drum in the six second interval or a generator speed greater than 500 cycles.

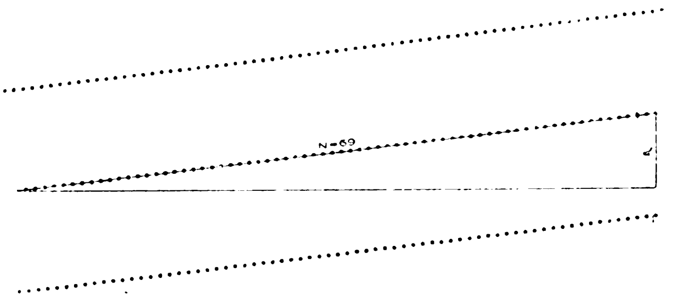


FIG 11

Assume for example that the the rows of dots are parallel to the axis of the drum. Then in 100 rotations a point on the surface of the drum would have traveled 100 times the circumference of the drum, 100 × 51.9 cm. This corresponds to a 500 cycle generator speed. By a simple proportion the generator frequency for any slope of line can be determined thus :

$$CN : CN \pm d = 500 : X$$

Where C = the circumference of drum.
N = number of rotations of the drum.
d = the perpendicular distance between the first and the last dot in a line.
X = the generator frequency.

The d above is the distance that the total run varies from being an even number of rotations of the drum. If the slope is up, d is negative; if down, the sign is positive.

Example :

C = 51.9
N = 100
d = 0.3 cm. Slope is up. Therefore d is minus.
Generator frequency = 499,971 cycles per second.

From the measurement of the crystal frequency by the standard wave meter, its value was found to be roughly 25 kc. Therefore it must have been the fiftieth multiple of the generator fundamental that was held during the run at the crystal fundamental frequency.

The crystal oscillator frequency is therefore fifty times the generator frequency or 24,998.55 cycles. If the generator speed should be increased sufficiently, a point will be found around 510 cycles where the zero beat condition may be again located by means of the phones. This is obviously the forty-ninth harmonic of the generator adjusted to the crystal frequency (510 × 49 = 25,000 approximately).

Accuracy of the method. — The accuracy of a calibration is chiefly dependent on the constancy to which the generator frequency can be held. To assist in this, the motor-generator is operated on a bank of high capacity storage batteries kept in good condition and fully charged. The machine is allowed to warm up thoroughly before a run is attemped. The Maxwell Bridge battery is also carefully checked for constant voltage, and the brushes and contactor segments in the Bridge circuit are kept clean and properly adjusted. As previously stated, slight variations in the generator frequency can be compensated for by the operator, but if the run is noticeably poor it is so recorded, or discarded.

The error arising from the measurement of the line d is minimized by determining it for all six parallel lines in a record, and using the mean as the correct value for d. The resulting error does not average more than one part in 200,000. The combined errors then, on a good run, are well within 0.001 °/₀. This degree of accuracy is possible only when the crystal is left undisturbed. The accuracy claimed for the standard crystal oscillator when used as a portable standard is only 0.002 °/₀. No error is observable due to a probable error in the measurement of the drum circumference.

To give an idea of the correspondence of different calibrations a table of eight consecutive runs on a standard crystal is appended. It will be noted that the probable error in the mean value is three parts in two and one half million.

Table of calibrations.

Date.	Temp. degree C.	Frequency, cycles.
8-15-27	38.45	24998.60
8-15-27	38.45	24998.75
8-16-27	38.45	24998.71
8-16-27	38.45	24998.73
8-29-27	38.55	24998.99
8-29-27	38.55	24998.82
9- 8-27	38.50	24998.71
9- 8-27	38.50	24998.63
	Mean value =	24998.74

Uses of the standard crystal controlled oscillator. — This standard oscillator is a portable device, the size being 8 × 14 × 22 1/2 inches. It is simple to use, there being no variables or tuning controls. It requires a plate potential of only ninety volts, and the heater may be operated on the 110 volt line. By its use, two hundred or more fixed frequencies equally spaced in kilocycles and accurate to about 0.001 °/₀ are made available. The permanence of the crystal calibration depends only on the permanence of the crystal itself.

This oscillator is used at the Naval Research Laboratory to standardize all quartz crystals manufactured there. Through the medium of a precision heterodyne frequency meter with a straight line frequency characteristic a crystal of any frequency can be calibrated from the standard oscillator to about 0.002 °/₀.

Furthermore, all heterodyne frequency meters for the Naval Service are calibrated directly against the harmonics of a standard oscillator. Readings may be taken as rapidly as the heterodyne frequency meter can be tuned to the successive harmonics of the crystal oscillator. For very accurate work, readings are taken on a beat note of 1,000 cycles (as determined by a tuning fork) on both sides of the zero beat point.

The calibration of a heterodyne frequency meter can be rapidly checked to determine the error at any time, by taking readings on a few harmonics of the crystal. For this purpose a special type of crystal oscillator without temperature control has been furnished the Fleet under the name of the « Crystal Controlled Calibrator », Navy Type # SE 2907.

The standard oscillator may also be used for the inter-comparison of frequency standards. One such comparison has already been made between the Navy's standard crystal controlled oscillator and the Bell Telephone Laboratories' standard tuning fork, to determine the relative constancy and the agreement in calibration of the two standards. For this test, the Navy oscillator was transported to New York City. A number of oscillographic records of the beat frequency between harmonics of the two standards taken at intervals over a period of three days indicated that there was no noticeable variation in the relative constancy of the two sources. The determination of frequency by the two standards differed by one part in 80,000 or 0.0012 °/₀. This agreement is considered excellent when it is remembered that the Navy standard had been taken from Washington to New York for the test and that entirely different methods were used to calibrate the two standards.

Since this comparison with the Bell Telephone Laboratories' standard was made, this Laboratory has built four crystal controlled oscillators similar to the one described in this paper, as a part of a program for the establishment of a national secondary frequency standard decided upon at a conference in Washington in February of this year. This conference was attended by representatives of the various interested government departments, and of a number of the radio manufacturing companies.

It is hoped that, after a determination of the frequency of these oscillators at several other laboratories has been made, and an average result arrived at, that these oscillators may serve as useful secondary standards of frequency for the government and the radio industry of the country.

* * *

The Directional Observations on Atmospherics in Japan,

by EITARO YOKOYAMA and TOMOZO NAKAI,
Electrotechnical Laboratory, Ministry of Communications,
Tokyo (Japan)

I. *Introduction.* — Since February 1927, daily observations have been carried out on the directions of such atmospherics as to disturb the reception of long wave signals in our branch Laboratory at Isohama Town which is situated about 100 kilometers north-east from Tokyo and facing the Pacific Ocean. Though the duration of the observations has been too short to reach definite conclusions, we avail ourselves of this opportunity to bring the results before the Congress.

II. *Apparatus and method of observations.* — A Telefunken E-276-b type direction finder has been in use. This is a heterodyne receiver which consists of a three-stage radio-frequency amplifier, a detector and a two-stage audio-frequency amplifier, in combination with some replaceable circular loops, which are 0.85 meter in diameter and can be rotated about a vertical axis, for obtaining a wide range of wave lengths and a small vertical antenna which may give the receiver a unidirectional property. As 7,200 meters was the longest wave length to be received by the equipment, another loop has been prepared to cover longer wave lengths necessary for the present observations, and the vertical antenna replaced by a longer one. The vertical antenna was coupled with the loop by its loading coil, the resonance condition being found by a variable condenser in parallel with the coil. The loop had also a tuning-in condenser. By this set, heart-shape characteristics of reception can be easily obtained, the phase of induced emf. being adjusted by the tuning of each circuit, and the antenna effect adjusted by the degree of coupling of the coil.

Atmospherics, especially those of the nature of clicks, generally change their strengths as well as their directions from moment to moment; thus it is not so easy to plot out their reliable polar curves for clicks or even for grinders. In order to simplify the procedure and to increase the accuracy in actual observations, a method of one-point observations has, therefore, been adopted, where a direction is first to be determined by turning the loop and by finding a minimum point of response; the sense is then to be decided by adding the vertical antenna to the loop, because a unidirectional combination will be liable to greater errors for direction finding than a loop only.

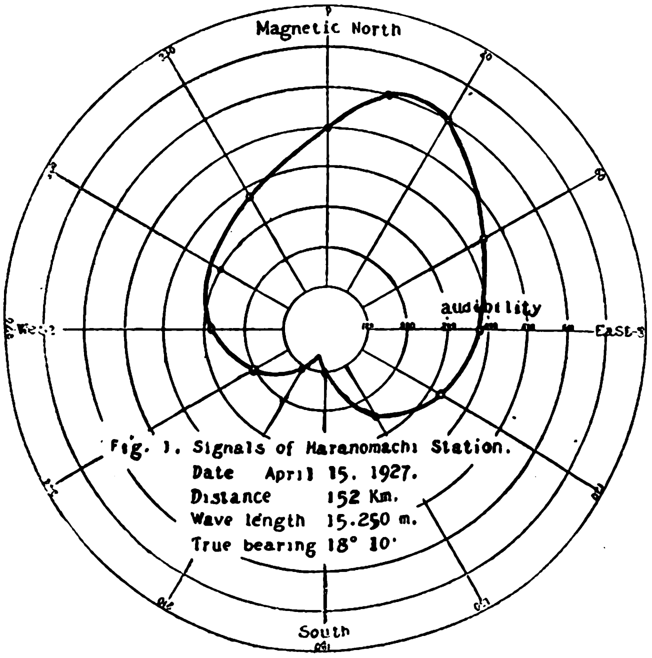


Fig. 1. Signals of Haranomachi Station.
Date April 15. 1927.
Distance 152 Km.
Wave length 15.250 m.
True bearing 18° 10'

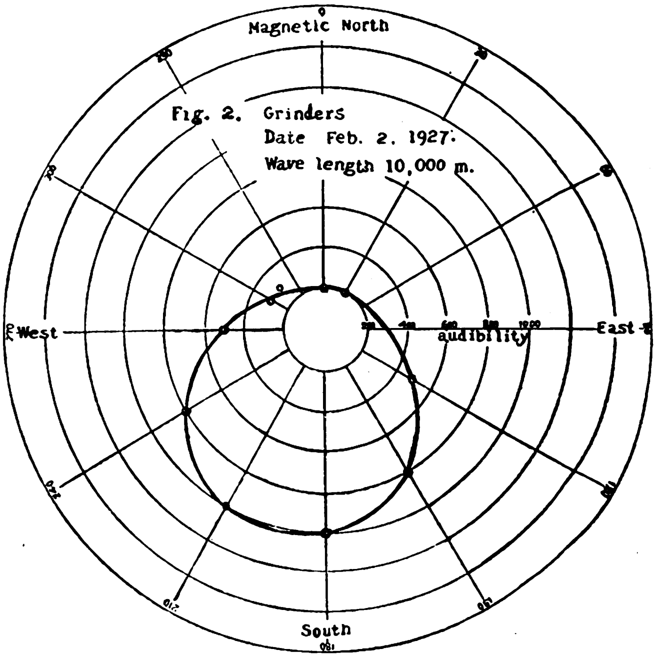
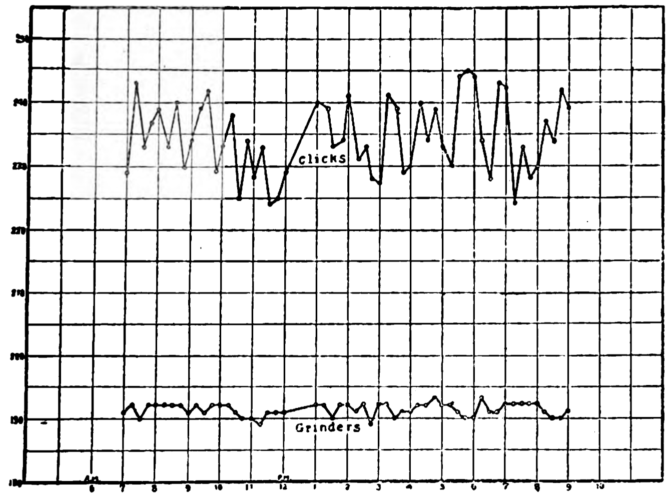


Fig. 2. Grinders
Date Feb. 2. 1927.
Wave length 10,000 m.

III. *Unidirectional polar curves.* — Fig. 1 is an example of the unidirectional polar curve for signal reception, and Fig. 2 that for grinders. As shown in the figure, no polar diagrams for atmospherics, as is well known, form heart-shapes at all, which is a phenomenon of interest for further studies.

IV. *Accuracy of the directional observations and correction for the apparatus.* — For the observations of strong signals, it has been found that the apparatus has an accuracy within one degree. But, for those of grinders, the band of minimum response was as wide as 2 degrees to 10 degrees and sometimes even reached 15 degrees, the width of the band depending upon the times of the day and the season, which might indicate that the seat of atmospherics covered a large area or moved from place to place. The middle points of the bands have, therefore, been taken as the directions of atmospherics. As clicks, which occur intermittently in short intervals of time and have much more variation in the observed directions, have been far more difficult for observations, it may be inferred that their accuracy has been much lower than those for grinders.



Japan Central Standard Time.

FIG. 3. — Diurnal variation in the directions of grinders and clicks in May 26, 1927. Wave length 10,000 m.

In addition to the above fact, there were present, in the surroundings of the measuring apparatus, several unremovable conductors which may give rise to some errors in the directional observations. For the rough correction of the effects, the bearings of several stations at known locations were observed and compared with their true bearings. It has thus been found that the maximum deviations were only one or two degrees, though the deviations were variable according to directions. On the figures in Table 1 below, no corrections have, therefore, been introduced.

Furthermore all the observations have been made by taking the bearings clockwise from the magnetic North in degrees. For finding out the similar bearings measured from the true North, the magnetic declination has to be taken into account. As the declination at the receiving station may be regarded as 5° 00' W. in average, corrections have, therefore, been made on the figures in table I below by substracting 5° from the observed bearings.

V. *Results of observations.* — 1) The observations were commenced at February 5. Until March 7, the observations of grinders only had been carried out on various wave lengths between 10,000 and 21,000 meters, about 10 times a day from 9.00 A.M. to 4.00 P.M. No appreciable variation due to wave lengths had however been noticeable, their amount being comparable to that of the diurnal variation observed on a particular wave length.

2) After the date, the observations have, therefore, been confined to two particular wave lengths, i.e., 10,000 meters and 20,000 meters. However no appreciable change in the observed directions has been noticed between the two.

3) After March 16, the time of observations has been extended from 7 A.M. to 9 P.M., observations being made at nearly every 30 minutes. The variation has, in general, been less in the day-time than at night, though it has not been so remarkable as in the case of signal observations. This seems to indicate that the seat of atmospherics is not invariable.

4) Since April, clicks have been observed in addition to grinders. The directions of clicks have been found to be obviously different from those of grinders, and the diurnal variation in the directions of clicks to be much greater than

those of grinders. One example of the diurnal variations in the directions of grinders and clicks is shown in Fig. 3. This seems to indicate that the seat of clicks differs from those of grinders.

5) From the observations on grinders from February to July, it may be said that they have not almost changed their directions during the season of the year and come from SSW (about 190°).

6) From the observations on clicks from April to July, it may be said on the contrary that their bearings have gradually shifted clockwise, though they were generally indistinct since the middle of June. This indistinctness seems to indicate that the seat of clicks is vastly extended in summer season.

The results of the observations are shown in Table I, the readings of directions being expressed in degrees measured clockwise from the North.

TABLE I. — *Directions of Atmospherics at Isohama.*

Date	Direction of Grinders			Direction of Clicks			Field Intensity of atmospherics in μ v/m.
J. C. S. T. (1)	Mean	Max.	Min.	Mean	Max.	Min.	
Feb., 5.	191	193	189		no obs.		
» 21.	191	197	185		»		
» 23.	189	192	180		»		
» 26.	190	193	187		»		
» 28.	191	193	190		»		
average =	190						grinders = 33 clicks = 1,198
March, 3.	190	192	187		no obs.		
» 7.	187	193	183		»		
» 9.	185	202	169		»		
» 16.	185	198	179		»		
» 24.	188	205	170		»		
» 31.	189	203	181		»		
average =	187						grinders = 26 clicks = 1,209
April, 7.	192	195	188	202	205	199	
» 14.	191	195	186	215	225	199	
» 15.	192	204	185	243	255	230	
» 16.	191	194	189	225	230	220	
» 21.	192	194	189	234	240	227	
» 28.	192	193	190	232	245	225	
average =	192		average =	225			grinders = 42 clicks = 2,050
May, 5.	191	193	189	225	250	205	
» 12.	192	193	190	237	251	223	
» 19.	191	193	189	236	247	225	
» 23.	192	194	190	235	245	227	
» 26.	191	193	189	235	245	224	
average =	192		average =	234			grinders = 52 clicks = 1,430
June, 2.	191	195	185	237	253	222	
» 9.	191	196	188	239	252	225	
» 23.	193	202	189	no distinct directions.			
» 27.	191	200	185	250	254	245	
» 30.	191	200	185	no distinct directions.			
average =	191		average =	242			grinders = 55 clicks = 3,100
July, 7.	190	199	180	directions obscure.			
» 14.	192	198	183		»		
» 21.	193	196	190		»		
» 23.	—	—	—	300	obscure.		
» 28.	192	194	190	270	280	260	
» 30.	—	—	—	270	280	260	
average =	192		average =	280			grinders = 59 clicks = 3,080

(1) Japan Central Standard Time.

To get an idea, in Table I are added the field intensities of atmospherics in microvolts per meter. The intensities have been daily measured with a different vertical antenna and the figures given in the table are the monthly average values (wave length = 15,600 m) measured at 4.30 P.M., by which atmospherics may be considered to be most prevalent. The intensities of grinders are those of signals which will be just readable through the grinders while the intensities of clicks are those of artificial clicks which can be regarded equal in strength to the natural clicks.

VI. *Results obtained by other observers.* — T. Nakagami of the Ministry of Communications, Tokyo, described in one of his papers regarding directions of atmospherics observed at the Osaka station in 1924-1925 as shown in Table II, in which the directions are expressed in angles measured clockwise from the North in degrees.

TABLE II. — *Directions of Atmospherics at Osaka.*

Month	Direction of Atmospherics at 18,000 m.
January	220° (S W)
February	200° (S S W)
March	200° (S S W)
April	250° (W S W)
May	315° (N W)
June	350° (N N W)
July	30° (N N E)
August	45° (N E)
September	40° (N N E)
October	20° (N N E)
November	300° (W N W)
December	240° (W S W)

J. Obata of the Aeronautical Research Institute, Tokyo Imperial University has also carried out some observations on atmospherics in Tokyo, commencing in July, 1926, the results of which are shown in Table III.

TABLE III. — *Directions of Atmospherics at Tokyo.*

Month	Direction of Atmospherics at 10,000 m.
August, 1926	N W — W N W
July, 1926	N W — W N W W N W — W
September, 1926	W N W — W
October, 1926	S S W
November, 1926	S S W
December, 1926	No obs.
January, 1927	S S W
February, 1927	S S W
March, 1927	S S W — S W
April, 1927	No obs.
May, 1927	No obs.
June, 1927	W

The results obtained by the above two observers will now be compared with ours. According to their observations, the bearings began to rotate clockwise from SSW in winter, arrived at the furthest in summer and then came back counter-clockwise until the original bearing was again reached in winter, one cycle being completed in one year. In our observations, the bearings for clicks have been tracing a similar variation, while those for grinders have remained unchanged. This fact will probably indicate that the two sets of observations above mentioned were those for clicks.

VII. *Conclusions.* — From the results of the observations mentioned above, the following conclusions may be arrived at.

1) No appreciable change has been found in the observed directions of atmospherics by the variation of wave lengths which lie between 10,000 and 20,000 meters.

2) On the diurnal observations 7 A.M. to 9 P.M., no appreciable regular change has been noticeable in the observed directions of atmospherics by the time of day. Though some expected variations have, in general, been noticed at sunrise, sunset and night-times, they have not been so remarkable as in the case of signal observations.

3) The directions of clicks have been obviously different from those of grinders, which seems to indicate that the seat of clicks differs from those of grinders.

4) The diurnal variation has been much greater in the directions of clicks than in those of grinders. This seems to indicate that the seat of clicks lies nearer to the observed region than those of grinders, if the seats of both kinds of atmospherics might be assumed to have nearly equal areas. Judged from the great differences

in the intensities, as well as in the nature of tones of clicks and grinders, this assumption may be considered to be probable.

5) The directions of clicks have shifted from nearly South toward West as the season advanced from winter to summer, while those of grinders have remained unchanged.

6) In summer, the clicks, which seem to originate from the mountain regions of Japan, seem to be most prevalent, while grinders probably come always from a tropical region such as the East-Indies.

Acknowledgement is due to K. Ichikawa and A. Moriyasu of this laboratory for their earnest assistance through the experimental works.

August, 1927.

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O. DAHL and L. A. GEBHARD, pp. 16-18.

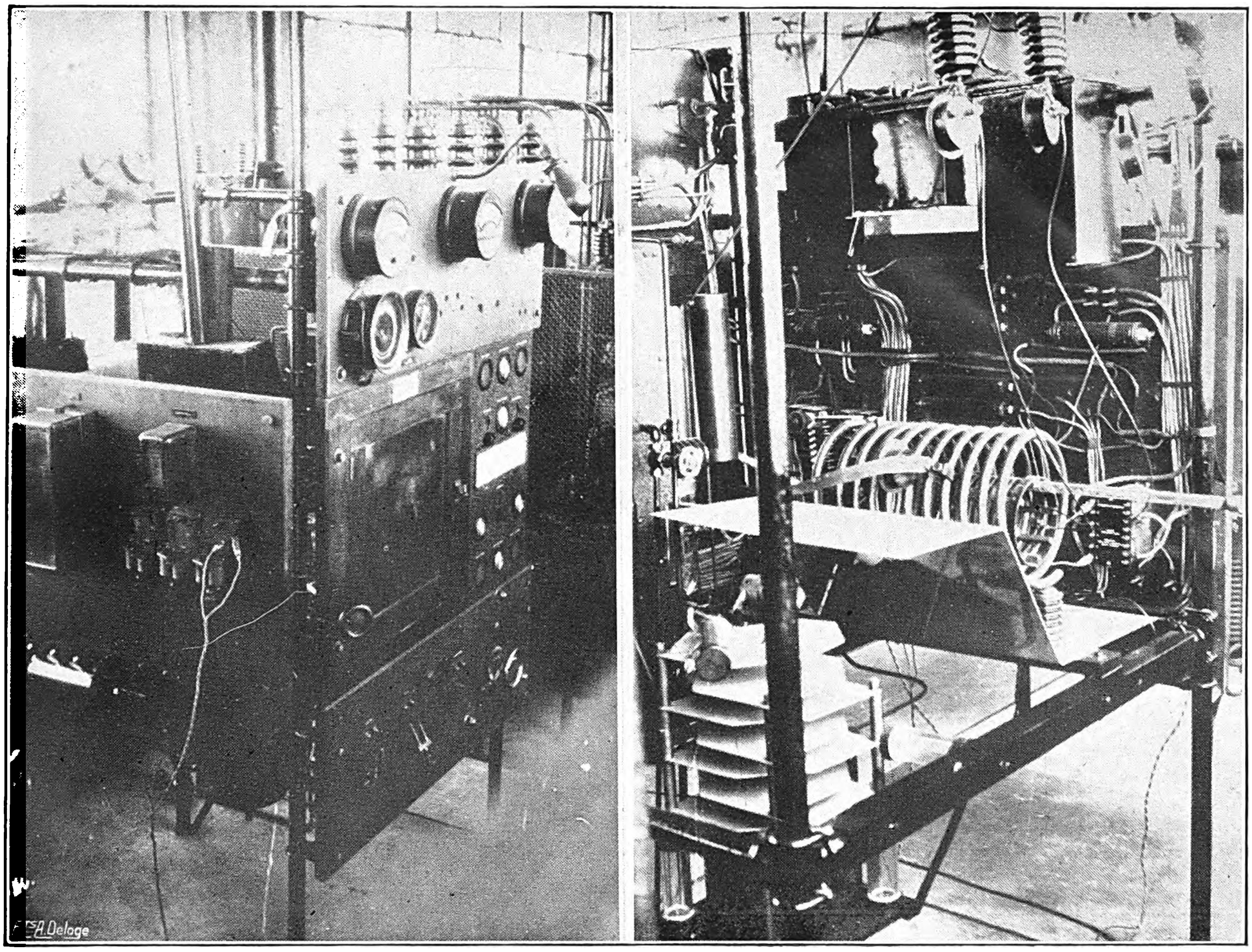

FIG. 3. — Views of transmitting apparatus.

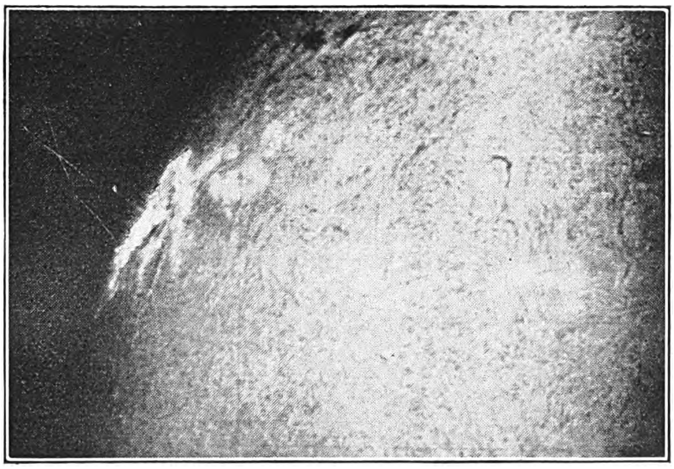

FIG. 6. — H-alpha image of sunspot-group photographed at Mount Wilson, August 19, 1927, $14^{h}44^{m}$ Greenwich mean time.

O. DAHL and L. A. GEBHARD, pp. 16-18.

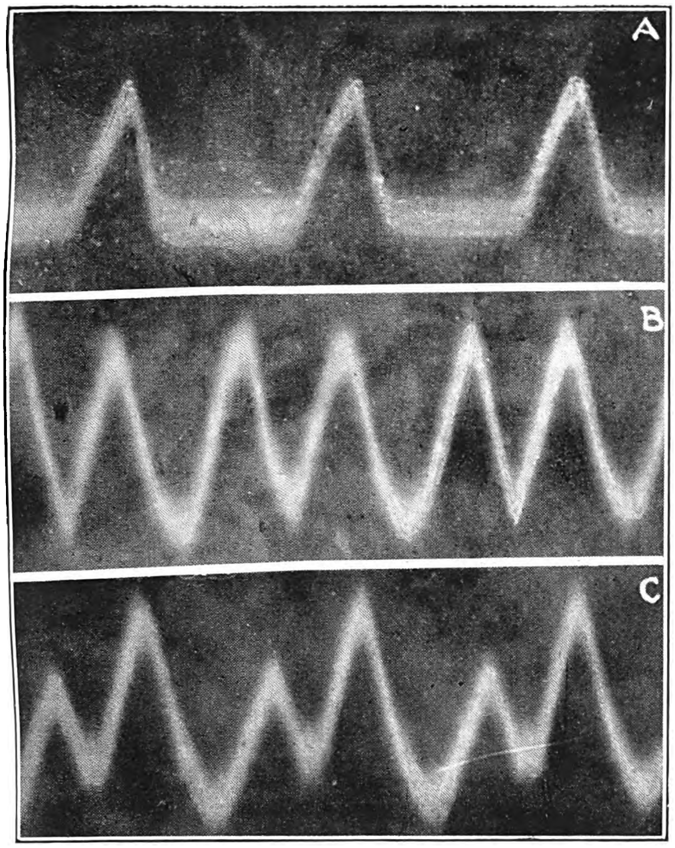

FIG. 4. — Wave-forms observed on August 25, 1927.

[(A) Reflection absent, showing wave-form of transmitter. — (B) Reflection approximately as strong as the ground-wave. — (C) Reflection weaker than ground-wave.]

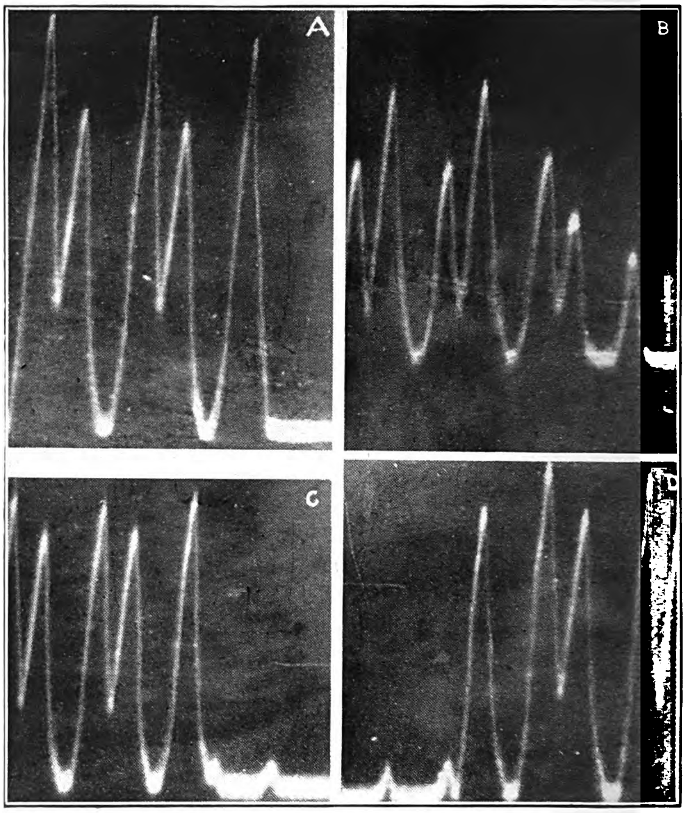

FIG. 5. — Wave-forms observed on October 8, 1927.

[(A) End of dot, reflection bigger than ground-wave, no key-click. — (B) End of dot, reflection smaller than ground-wave, last ground-hump weaker than preceding ones as also its reflection and in the right proportion. — (C) End of dot, reflection slightly bigger than ground-wave; note small key-click giving diminutive ground and reflected humps; time-separation between clicks same as between big humps. — (D) Beginning of dot, key-click arrives first along ground, then as a reflection, then big ground-hump, its reflection, etc.; separation checks as in (C). Note that amplitude of ground-hump is constant for all.]

G. FERRIE et R. JOUAUST, pp. 21-28.

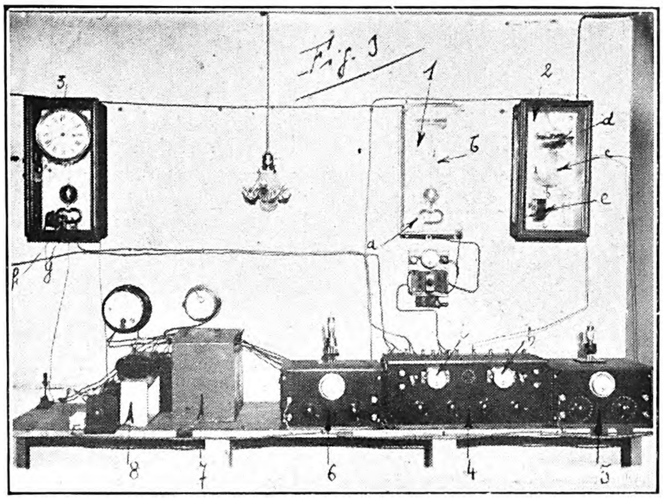

FIG. 3.

1. Pendule libre auto-entretenu.
   - *a*) Enroulement d'entretien.
   - *b*) Miroir.

2. Dispositif.
   - Cellule photoélectrique (*c*).
   - Source lumineuse (*d*).
   - Volet d'escamotage (*e*).

3. Pendulette compteur avec sa bobine d'auto-entretien (*f*), et ses bobines de synchronisation (*g*).

4. Boîte d'amplification du courant photoélectrique.
   - *h*) Milliampèremètre du courant d'entretien du pendule libre.
   - *i*) Milliampèremètre du courant de synchronisation de la pendulette.

5. Boîte d'alimentation de la tension cellule sur le courant alternatif.

6. Boîte d'alimentation de la tension plaque des lampes de l'amplificateur sur le courant alternatif.

7. Accumulateurs de chauffage des lampes de (4) et de manœuvre du volet (*e*).

8. Dispositif de charge continue des accumulateurs sur le courant alternatif.

E. B. JUDSON, pp. 38-40.

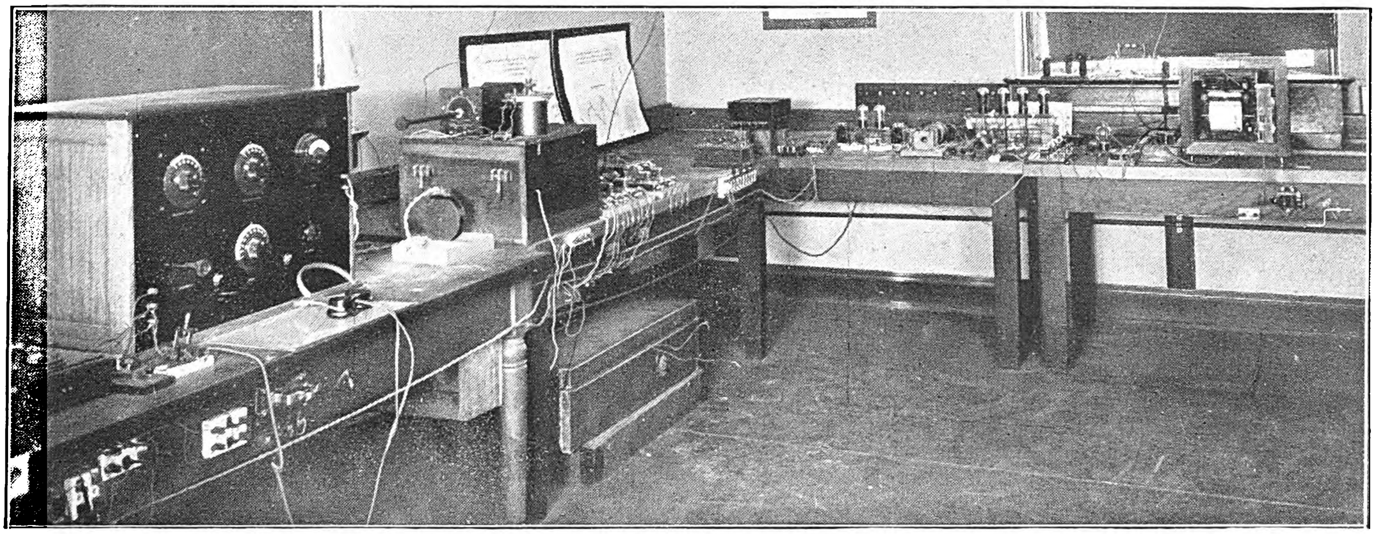

Fig. 2.

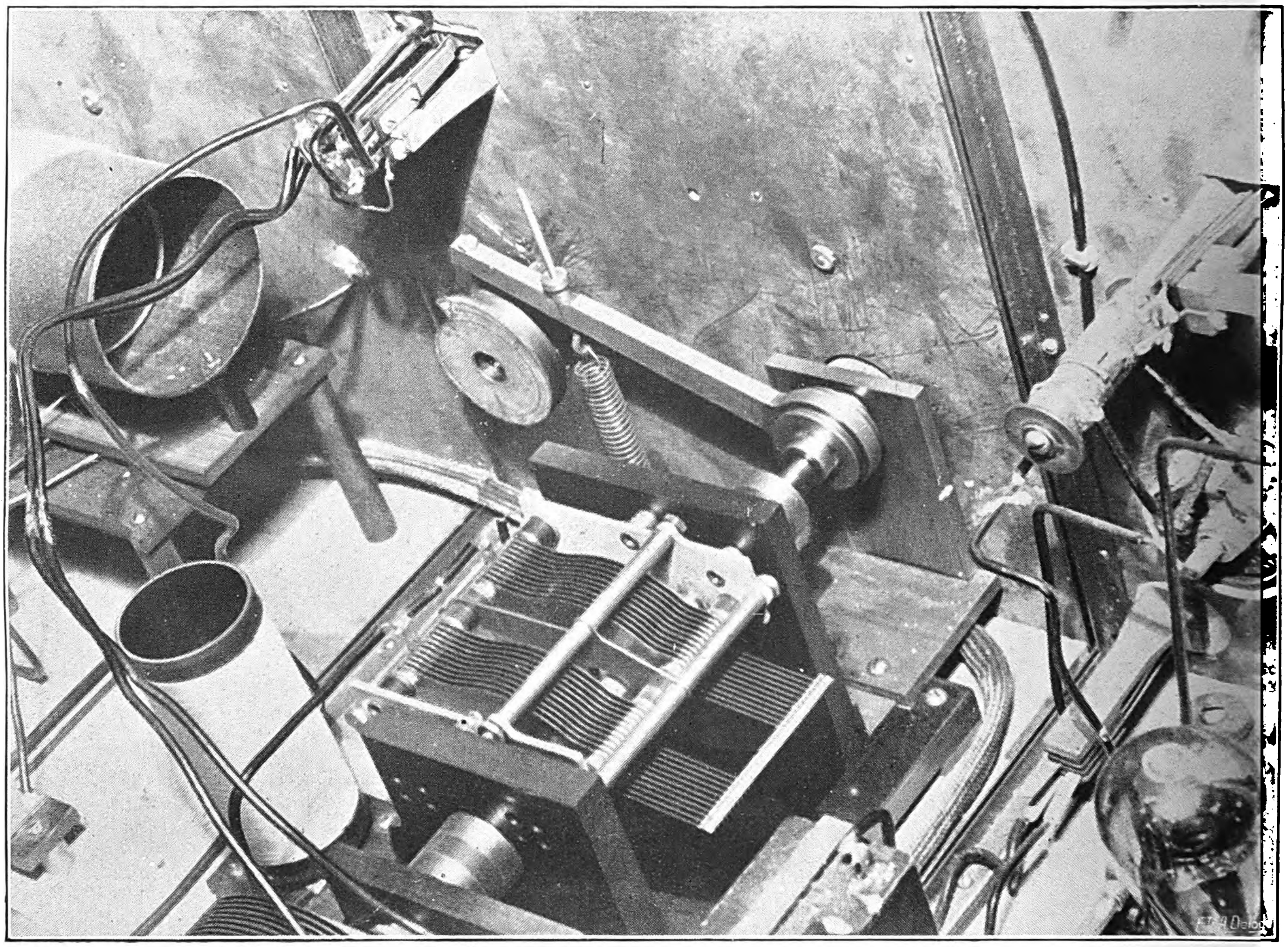

FIG. 8.
View of Cam and Main Tuning Condenser.

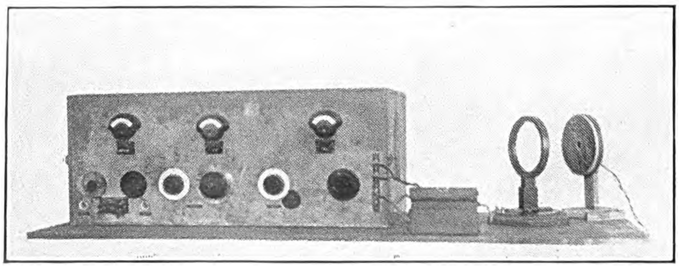

FIG. 9.
External View of the Completed Apparatus.

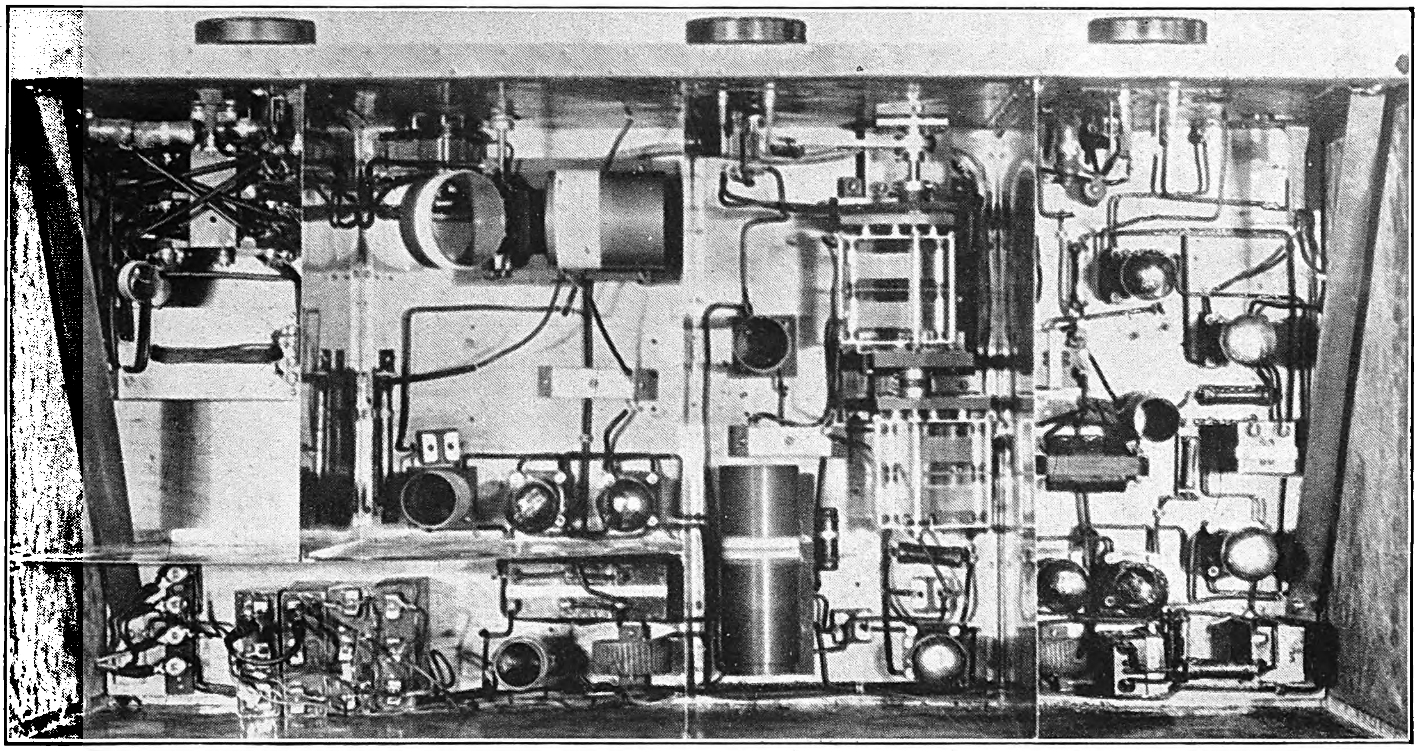

Fig. 10.

Internal View of the Complete Apparatus.

Robert H. WORRALL and Raymond B. OWENS, pp. 67-73.

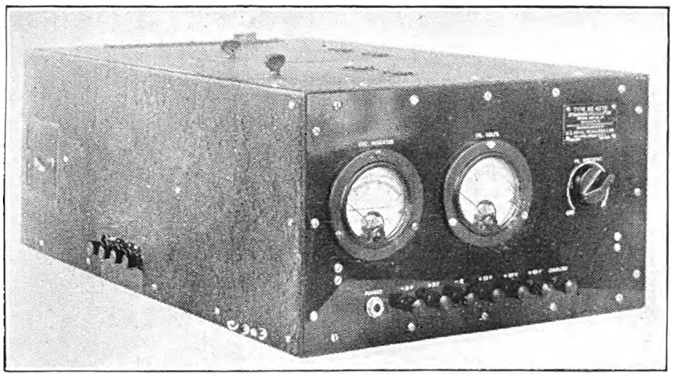

Fig. 5.

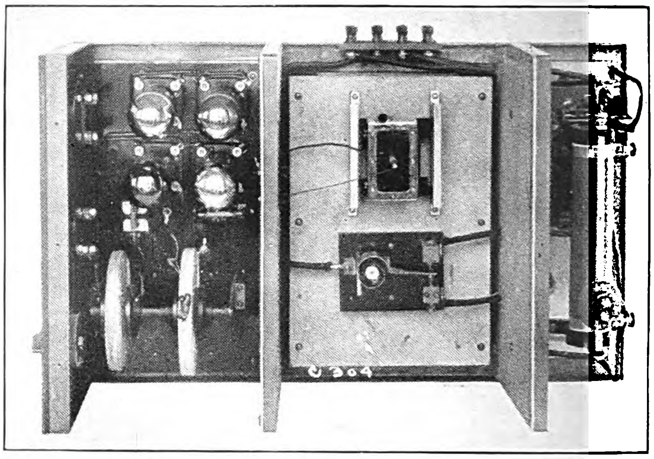

Fig. 6.

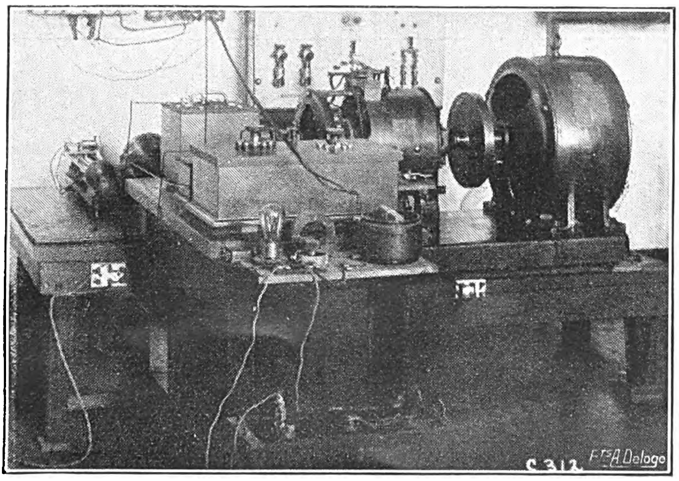
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Fig. 7.

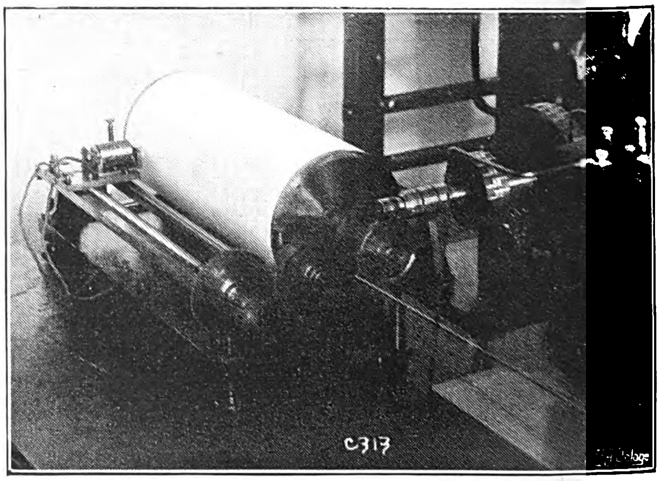
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Fig. 8.

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SCIENTIFIC PAPERS
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American Section 4 Feb. 1929 [illegible] 1bis, [illegible] 6

L'existence de plus d'une couche ionisée dans la haute atmosphère,

par le Profr E. V. APPLETON, F. R. S.
(Angleterre)

Depuis un an et demi, des observations systématiques ont été effectuées à la Peterborough Research Station sur les caractères des ondes radiotélégraphiques déviées par la haute atmosphère. Parmi ces mesures régulières, on s'appliqua à de nombreuses déterminations de la hauteur effective de la couche de Kennelly-Heaviside, en utilisant des émissions spéciales du National Physical Laboratory et des postes de la British Broadcasting Corporation. Les observations du début de l'été de 1926 montrèrent que, pendant la nuit, la couche déviante (appelée, pour la facilité, la couche E), avait habituellement, pour des longueurs d'onde de 400 mètres, une hauteur de 90 kilomètres à 130 kilomètres. Pendant la période allant d'octobre 1926 à mai 1927, on trouva cependant à maintes reprises des hauteurs d'un tout autre ordre de grandeur, savoir 250 kilomètres à 350 kilomètres, pendant les trois heures qui précèdent l'aurore.

Dans ces cas, après avoir enregistré pendant deux ou trois heures ces valeurs élevées, on constate une discontinuité dans les séries de valeurs 20 à 40 minutes avant le lever du soleil, et l'on enregistre à nouveau les valeurs normales de la hauteur.

Cette expérience montre que, très probablement, en de telles circonstances, pendant la période précédant l'aube, l'ionisation dans la couche de Kennelly-Heaviside (couche E) a été suffisamment réduite par la recombinaison, pour permettre à des ondes de cette fréquence d'y pénétrer. La « réflexion » se produit cependant sur une couche plus haute (couche F) dont le degré d'ionisation est plus grand. Au lever du soleil, à une hauteur d'environ 100 kilomètres, la couche de Kennelly-Heaviside (couche E) se reforme et la déviation due à cette couche s'établit brusquement. La chute normale de sa limite inférieure se poursuit lorsque l'action plus directe du soleil agrandit la région ionisée. Le cours de la journée se poursuivant, les résultats expérimentaux montrent qu'il se forme, au-dessous de la couche de Kennelly-Heaviside, une troisième région d'ionisation (couche D) qui provoque l'affaiblissement des ondes, mais n'affecte matériellement pas la hauteur à laquelle elles sont déviées. On détecte occasionnellement des ondes « réfléchies » par cette couche. Sa fonction principale est, cependant, d'absorber les ondes, qui ne sont que légèrement réfractées en la traversant, et qui sont finalement déviées par la couche E. Nous croyons que la couche D n'est autre que la couche d'ozone découverte par Lindemann et Dobson.

* * *

L'influence du champ magnétique terrestre sur les transmissions radiotélégraphiques

(Résumé),

par le Profr E. V. APPLETON, F. R. S.
(Angleterre)

La théorie magnéto-ionique de la propagation des ondes radiotélégraphiques dans un milieu ionisé proposée par l'auteur (*) et par Nichols et Schelleng (**) est étendue au cas général de la propagation dans toutes les directions par rapport au champ magnétique. Les symboles utilisés ont la même signification que ceux employés par H.-A. Lorentz (***) avec cette différence que, pour des électrons libres, on montre qu'il faut écrire $g = \frac{m}{\tau}$ (et non $g = \frac{2m}{\tau}$ comme le fait Lorentz pour des électrons liés). Lorsqu'on néglige l'absorption, la formule pour l'indice de réfraction se réduit à

$$\mu^2 = 1 + \frac{2}{2\alpha - \frac{\gamma_T^2}{1+\alpha} \pm \sqrt{\frac{\gamma_T^2}{(1+\alpha)^2} + 4\gamma_L^2}}$$

ou γ_T et γ_L se rapportent au termes comprenant respectivement les composantes tangentielle et longitudinale du champ magnétique. Il y a donc, en général, deux ondes qui se propagent.

(*) *Proc. Phys. Soc.*, 37, February 1925.

(**) *Bell Technical Journ.* 4, 215, April 1925.

(***) « *Theory of Electrons* », pp. 139 et seq.

La formule ci-dessus montre que μ^2 devient infini quand

$$\alpha^2 = \gamma_L^2 + \left(\frac{\alpha}{1+\alpha}\right)\gamma_T^2$$

de telle sorte que c'est seulement quand on a $\alpha \gg 1$ qu'on obtient

$$\alpha^2 = \gamma_L^2 + \gamma_T^2 = \gamma^2$$

et qu'il y aura quasi-résonance avec le champ total. Cela signifie qu'il ne peut se produire d'effets critiques pour une onde (200 mètres environ) que si l'ionisation est faible.

La formule montre en outre que μ^2 s'annule, ce qui indique la possibilité de la réflexion même à une incidence verticale sur la couche, quand

$$1 + \alpha = 0$$

ou

$$1 + \alpha = \pm \gamma,$$

relations dans lesquelles n'entre que le champ magnétique total.

L'absorption dans la couche D, agira différemment sur les deux ondes qui sont, en général, transmises, de telle sorte que pour les longueurs d'onde de l'échelle de la radiodiffusion en Europe, nous pouvons nous attendre à ce que, seul, le rayon non-critique atteigne le sol avec une intensité appréciable. De cette façon, les résultats de Meissner, qui ne trouva pas d'anomalies à 212 mètres, peuvent s'expliquer.

* * *

De l'influence de l'activité solaire sur la radio-transmission (1),

par L. W. AUSTIN et Miss I. J. WYMORE

Laboratory for Special Radio Transmission Research,
Bureau of Standards, Washington D. C. (U. S. A.)

A la réunion de la Section Américaine de l'U. R. S. I. en avril 1927, j'ai montré quelques courbes (2) d'où ressort une relation entre l'intensité de la réception transatlantique des ondes très longues et l'activité solaire mesurée par les nombres de taches solaires et par les valeurs de la constante solaire. Ces courbes représentaient des moyennes annuelles depuis 1915 jusqu'à 1926 et des moyennes mensuelles depuis 1922 jusqu'à 1926. Elles montraient, sans grande hésitation possible, qu'il existe une corrélation réelle entre l'activité solaire et l'intensité des signaux diurnes, losque les moyennes recouvrent de longues périodes. On pouvait voir également des courbes illustrant la correspondance entre les moyennes périodiques de taches solaires pour 27 jours et l'intensité de la réception.

G. W. Pickard (1) avait publié antérieurement un mémoire où il utilisait ses propres observations effectuées dans l'échelle de la radiodiffusion aussi bien que des observations émanant d'autres auteurs, depuis les ondes très longues jusqu'aux ultra-courtes. Dans ce mémoire, il démontra nettement que les phénomènes radioélectriques dépendent de l'activité solaire. Les observations personnelles de M. Pickard établissaient l'existence d'une relation inverse entre les signaux nocturnes dans l'échelle de la radiodiffusion et les nombres de taches solaires, alors que les signaux nocturnes de longueurs d'onde ultra-courtes traités par la même méthode, indiquaient une relation directe. Cette dernière conclusion ne paraît pas cependant conforme aux résultats d'un certain nombre d'observations, faites par d'autres, sur les signaux nocturnes affaiblis de longueurs d'onde ultra-courtes, à des époques de grandes perturbations solaire et magnétique.

Pendant l'été dernier, on a poursuivi, au Bureau of Standards, l'examen des résultats d'observations sur les ondes longues en rapport avec les taches solaires. On accorde maintenant une attention toute spéciale à la corrélation indiquée par les observations quotidiennes, ou par les moyennes établies sur quelques jours au plus, plutôt qu'aux moyennes de longues périodes. On trouve, en général, que les comparaisons diurnes des signaux et des taches solaires, donnent des courbes trop complexes pour pouvoir être utilisées. Mais, quand on en prend les moyennes pour des périodes de cinq jours, on commence à apercevoir de bons résultats. On trouve également, en ce qui concerne le matériel, que les stations situées à des distances modérées (moins de 1,000 kilomètres) donnent dans l'ensemble, lorsqu'on opère de la sorte une meilleure corrélation avec les taches solaires que les stations plus éloignées.

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(1) Lu à la réunion internationale de l'Union Internationale de Radiotélégraphie Scientifique à Washington, le 14 octobre 1927.

(2) Proc. I. R. E., vol. XV, p. 825, 1927.

(1) Proc. I. R. E., vol. XV, p. 83, 1927. Voir aussi vol. XV, p. 749, 1927.

Comme les courbes des signaux et des taches solaires ne se correspondent pas dans le détail, il semble probable qu'il existe d'autres facteurs, en dehors de l'activité solaire, qui contribuent à produire les variations dans les signaux.

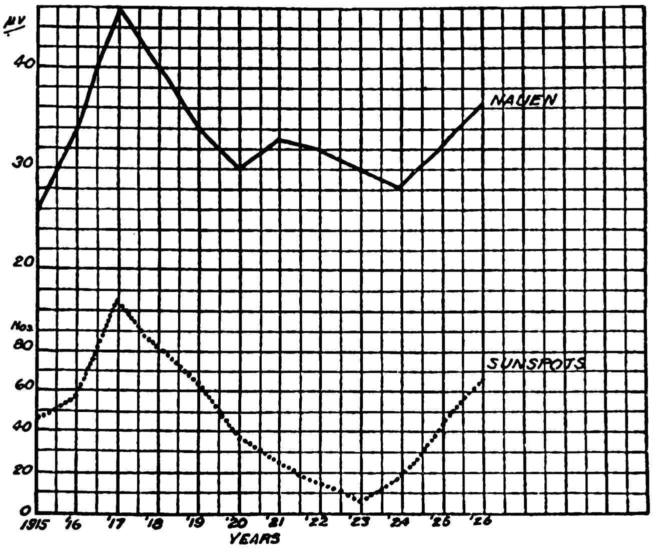

Fig. 1. — Moyenne annuelle de l'intensité de signal de Nauen (AGS), à 10 heures du matin, et nombres de taches solaires.

Nous pensons que l'un deux est la température (1) dont on a toujours montré la forte influence, surtout l'hiver, sur la réception à Washington des stations transatlantiques de Nouveau-Brunswick et de Tuckerton, New-Jersey.

Il semble encore y avoir une tendance, dans les courbes relatives aux taches solaires et les courbes de l'intensité des signaux, à présenter l'une par rapport à l'autre un déphasage tantôt en avant et tantôt en arrière, comme s'il y avait parfois un retard dans l'apparition des taches solaires par rapport à la perturbation solaire fondamentale qui les produit, et parfois un retard dans la formation des conditions atmosphériques terrestres qui produisent les variations dans l'intensité des signaux.

Malgré ces variations de phases, la fréquente similitude des courbes relatives aux taches solaires avec celles relatives aux signaux, confirme que nous sommes bien en présence d'une véritable relation. Dans l'ensemble, il paraît y avoir le même degré de corrélation entre les signaux et les taches solaires qu'entre les intensités des signaux de même longueur d'onde de deux stations émettrices distinctes reçus à une station réceptrice donnée.

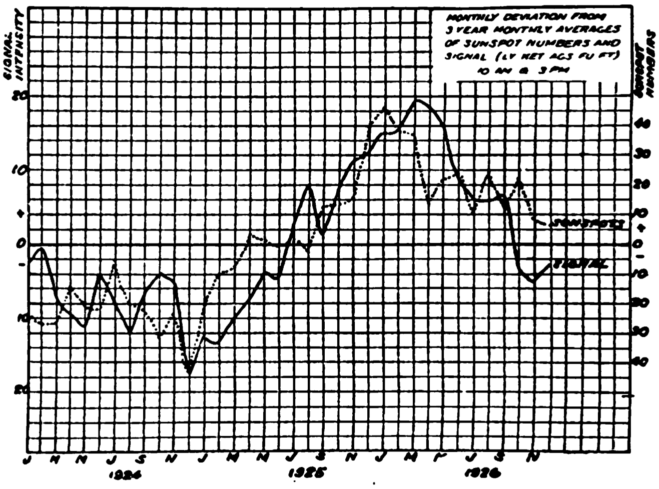

Fig. 2.

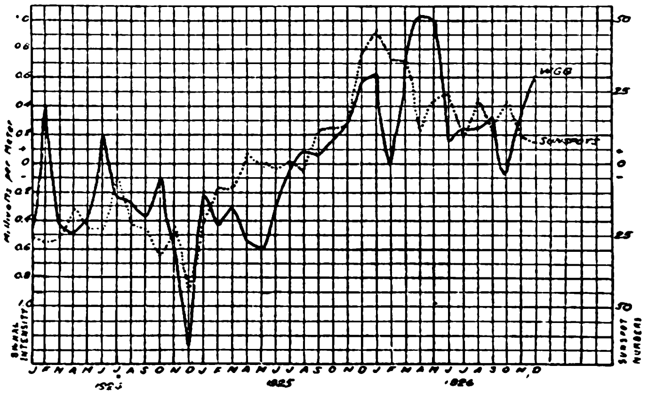

Fig. 3. — Ecarts entre les moyennes mensuelles de Tuckerton (WGG), à 10 heures et à 15 heures, et les moyennes mensuelles générales pendant trois ans (corrigées pour les variations dans le courant d'antenne) et nombres de taches solaires, 1924-1926.

Dans ce qui suit, les figures 1, 2 et 3 se rapportent à des durées de comparaison longues, dans lesquelles les liens entre l'activité solaire et les radioémissions semblent parfaitement bien établies, tandis que les figures 4, 5, 6 et 7 se rapportent à des moyennes sur de courtes périodes (5 jours) dans lesquelles les courbes de transmission sont interpolées (smoothed) par la formule d'interpolation $\frac{a+2b+c}{4}$. Cette dernière ne change pas la position des principaux maxima et minima, et rend les courbes de transmission plus facilement comparables avec celles des nombres des taches solaires.

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(1) Proc. I. R. E., vol. XIV, p. 781, 1926.

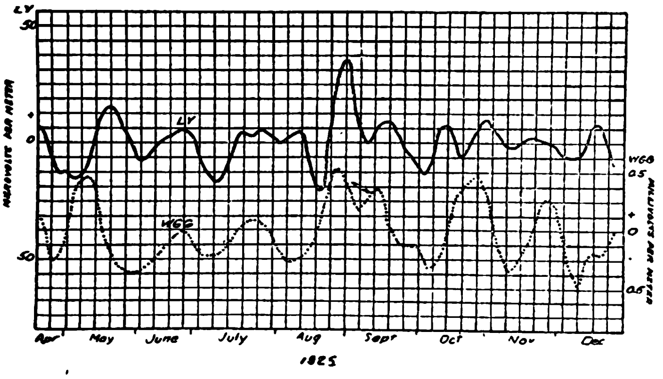

Fig. 4. — Ecart entre les moyennes de cinq jours et les moyennes mensuelles de Tuckerton (WGG), à 10 heures et à 15 heures, et Lafayette (LY), à 10 heures (1925).

La figure 1 représente les moyennes annuelles du poste de Nauen (λ = 13,000 mètres) tel qu'il est reçu à Washington à 10 heures du matin de 1915 à 1926, et les moyennes annuelles correspondantes des nombres de Wolfer (Wolfer sunspot numbers) s'étendant sur la même période. Les signaux de 1915 à 1921 furent mesurés par la méthode du téléphone shunté, donc moins exactement que les signaux pris par la suite; mais je pense qu'il est hors de doute que l'allure générale de la courbe est exacte.

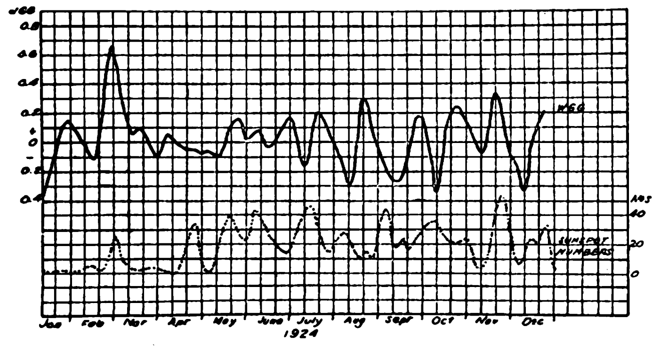

Fig. 5. — Ecart entre les moyennes de cinq jours et les moyennes mensuelles de Tuckerton (WGG), à 10 heures et à 15 heures, et moyennes de cinq jours des nombres de taches solaires, 1924.

La figure 2 (1) représente les écarts entre les moyennes mensuelles individuelles de cinq stations éloignées, et les moyennes mensuelles générales pendant trois ans, par comparaison avec les nombres de taches solaires. On voit que, bien que les deux courbes ne se correspondent pas dans le détail, elles s'élèvent en général ensemble pendant les trois années d'activité solaire croissante. La partie de la figure qui se rapporte à la croissance rapide des tâches solaires en automne 1925 est spécialement digne d'intérêt.

La figure 3 représente des courbes similaires pour la station transatlantique de la Radio Corporation à Tuckerton, N. J. (WGG), à 250 kilomètres seulement de Washington.

Comme on l'a dit, le degré de corrélation entre les taches solaires et l'intensité des signaux peut se comparer à celui qui existe entre les intensités de deux signaux reçus en un point et provenant de deux stations émettrices distinctes. A titre de comparaison avec les courbes suivantes la figure 4 montre la relation des moyennes de cinq jours de Tuckerton (WGG) et de Bordeaux (LY) telles qu'on a reçu ces stations à 10 heures du matin au Bureau of Standards, pendant une partie de 1925.

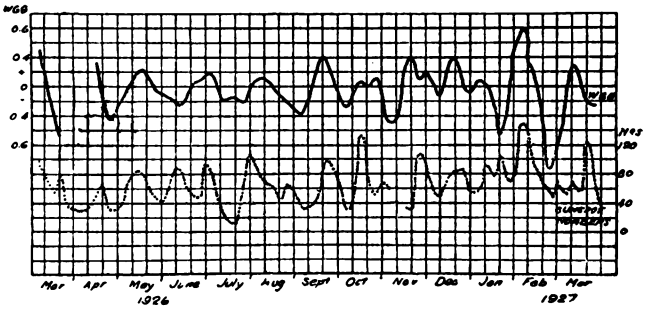

Fig. 6. — Ecart entre les moyennes de cinq jours et les moyennes mensuelles de Tuckerton (WGG) et moyennes de cinq jours des taches solaires, 1926-1927.

La figure 5 représente les écarts des moyennes de cinq jours de Tuckerton comparés aux moyennes de cinq jours des taches solaires en 1924. L'année 1924 correspondait presque au minimum des taches solaires.

La figure 6 représente la relation entre les taches solaires et les signaux de la même station entre mars 1926 et mars 1927. Ici, les courbes sont dessinées à une échelle plus petite qu'en 1924 à cause de la plus grande amplitude des variations du nombre de taches solaires et de l'intensité des signaux. On voit en même temps que le degré de corrélation des nombres de taches solaires et des signaux semble bien meilleur en 1926 et 1927 que pendant la période de faible activité en 1924.

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(1) Les figures 1 et 2 sont copiées du Proc. I. R. E., vol. XV, p. 825, 1927.

La figure 7 représente des courbes similaires en 1926-1927 pour la station W. S. S. de la R. C. A. à Rocky Point, L. I., distante de 435 kilomètres de Washington. En plus des courbes d'intensité de signaux et du nombre de taches solaires, on a fait figurer une courbe des moyennes de cinq jours de l'amplitude de la composante horizontale du champ magnétique terrestre observée à Cheltenham, Md. La similitude entre ces courbes est réellement frappante, bien que nous trouvions encore ici les déphasages déjà mentionnés entre les variations du nombre de taches solaires et celles de l'intensité des signaux. Dans certains cas, les variations des signaux sont en avance, alors qu'en d'autres cas, elles sont en retard.

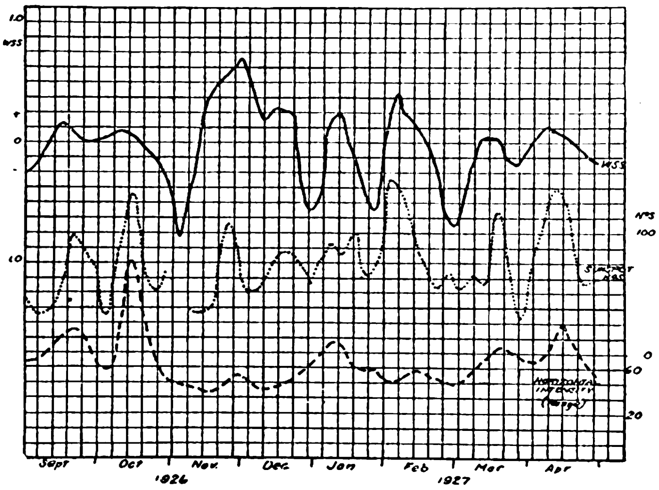

Fig. 7. — Ecart entre les moyennes de cinq jours de Rocky Point (WSS) et les moyennes mensuelles, à 10 heures et à 15 heures, et moyennes de cinq jours des nombres de taches solaires et intensité de la composante horizontale (Cheltenham), 1926-1927.

En examinant les courbes des moyennes de cinq jours, on peut se convaincre que, s'il peut être question d'un rapport entre l'activité solaire et l'intensité des radiosignaux, il n'y a pourtant pas de proportionnalité directe entre les nombres des taches solaires et l'intensité des signaux. Ce n'est en aucune sorte le plus grand accroissement des taches solaires qui produit toujours la plus grande amélioration dans la radiotransmission.

L'état actuel de nos connaissances de la relation entre l'activité solaire et l'intensité des radiotransmissions peut être résumée brièvement comme suit :

Il semble qu'on puisse raisonnablement affirmer que l'intensité des signaux diurnes à grandes longueurs d'onde a augmenté, ces dernières années, avec l'accroissement de l'activité solaire, lorsque la moyenne est prise pour des périodes d'un mois ou plus, et il est évident que les moyennes annuelles de l'intensité des signaux ont suivi grossièrement la courbe des nombres des taches solaires depuis 1915.

On peut tout aussi raisonnablement affirmer que dans la gamme de radiodiffusion, la transmission nocturne est affaiblie aux époques où les nombres de taches solaires sont élevés, les moyennes étant prises pour des périodes de quelques jours.

Les résultats relatifs à l'influence du soleil sur la transmission diurne des ondes longues et la transmission nocturne des ondes ultracourtes, quand les moyennes sont prises pour des périodes de quelques jours, sont quelque peu discordants suivant les différents moments et les différentes stations. Ces désaccords peuvent être dûs aux déphasages déjà mentionnés entre les courbes de la transmission et celles des taches solaires.

Très peu d'observations ont été publiées au sujet de la transmission nocturne des ondes longues et de la transmission diurne en radiodiffusion; aucune observation, à ma connaissance, sur la transmission nocturne en radiodiffusion, ne s'étend sur une période de plus d'un an.

On observe en général une similitude grossière entre les courbes de l'intensité de la réception de diverses stations à ondes longues se trouvant dans différentes directions autour de Washington. Cela semblerait impliquer soit qu'il y ait des variations simultanées dans les conditions électriques dans la haute atmosphère sur de très grandes étendues, soit que les conditions au voisinage immédiat de la station réceptrice constituent le principal facteur de l'intensité des signaux.

Occasionnellement, cependant, des différences marquées ont été observées dans la réception de stations de directions diverses, phénomène beaucoup plus commun dans le cas de la radiodiffusion et des ondes ultra-courtes reçues de nuit.

Si les nombres de taches solaires indiquent grossièrement les conditions qui règlent la transmission, il semble probable néanmoins qu'ils ne révèlent qu'incomplètement la forme particulière de l'activité solaire, qui est apparemment l'agent principal influençant les phénomènes radioélectriques.

* * *

Relations between Atmospherics and Meteorological Phenomena,

by Robert BUREAU.

As the paper of Captain Bureau did not reach us in time, it has not been possible to insert the translation here. This transmission will appear shortly in an additional Part.

* * *

Mesure de la hauteur réelle de la couche conductrice et des perturbations du 19 août 1927 (1),

par O. DAHL et L. A. GEBHARD,
Carnegie Institution of Washington (U. S. A.)

Méthode générale. — Les expériences décrites ci-dessous furent entreprises pendant l'été 1927. La méthode est celle décrite par Breit et Tuve (2). Comme antérieurement, les signaux furent émis par le « Naval Research Laboratory » et reçus par le « Department of Terrestrial Magnetism ».

Les dispositifs expérimentaux furent pour la plupart identiques à ceux employés par Breit et Tuve. Le schéma général fut le suivant : Un circuit commandé par un crystal, d'une fréquence de 4,015 kilocycles et d'une puissance de 10 kilowatts, situé à Bellevue, était modulé de manière à émettre de courts trains d'ondes interrompus. La durée de chacun des trains était grossièrement de 1/1,500 de seconde, l'intervalle entre la fin d'un train et le début du suivant étant d'environ 1/750 de seconde. Les signaux étaient reçus sur le toit du bâtiment principal du « Department of Terrestrial Magnetism ». Pour la réception un petit cadre était relié au premier détecteur d'un superhétérodyne ayant une fréquence intermédiaire de 50 kilocycles. Le second détecteur du superhétérodyne était alimenté à travers une résistance de 25,000 ohms connectée entre les filaments et les grilles de quatre lampes de 7.5 watts (n° 210 x) utilisées en parallèle. Le débit de ces lampes traversait un oscillographe de la firme General Electric, le courant de plaque constant étant compensé (1). La forme des ondes résultante était photographiée et observée visuellement.

L'idée de la méthode est de photographier sur un même dispositif d'enregistrement les trains d'ondes arrivant directement de Bellevue le long du sol ainsi que les ondes réfléchies par la couche conductrice. L'intervalle entre l'arrivée des ondes réfléchies et des ondes directes donne la hauteur effective de la couche.

Emetteur. — La figure 1 représente un schéma de l'émetteur. On utilisa comme source (master) un oscillateur à cristal de quartz. Son débit était amplifié une première fois par une lampe de 250 watts et ensuite par une lampe de 20 kilowatts. Le contrôle et la modulation s'effectuaient au primaire, c'est-à-dire à l'étage d'amplification de 250 watts. Pour faire ce contrôle, la clé étant levée, on appliquait à la grille une tension négative élevée. Cela empêchait la lampe de 250 watts d'agir. On utilisait le même principe pour la modulation.

La tension de modulation était obtenue à partir d'un générateur à 500 périodes alimentant un transformateur. Le courant à 500 périodes du transformateur passait à travers un potentiomètre, et une partie de la tension de grille était superposée à une tension constante négative. La figure 2 représente les dispositions actuelles. La tension négative constante était de 800 volts. La tension alternative à 500 périodes avait une valeur efficace de 525 volts. La courbe (*b*) en représente l'allure en fonction du temps. La courbe (*a*) montre les intervalles de temps pendant lesquels la lampe de 250 watts agit. Les ressauts qu'on aperçoit en (*a*) représentent approximativement l'enveloppe du courant d'antenne.

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(1) Présenté devant la Conférence de l'Union Internationale de Radiotélégraphie Scientifique, le 14 octobre 1927.

(2) G. Breit et M. A. Tuve (*Phys. Rev.*, vol. XXVIII, 1926, pp. 554-575).

(1) Le texte anglais porte : « The output of these tubes was passed through a General Electric oscillograph with a proper balancing arrangement for the steady plate-current. »

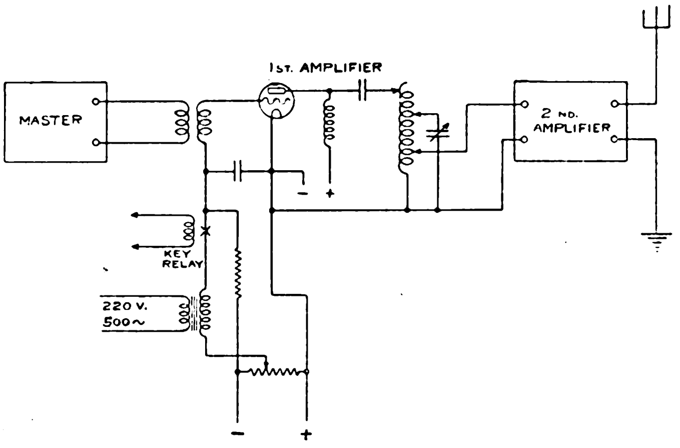

Fig. 1. — Schéma du circuit d'émission contrôlé par un cristal, 10 kilowatts, 4,015 kilocycles.

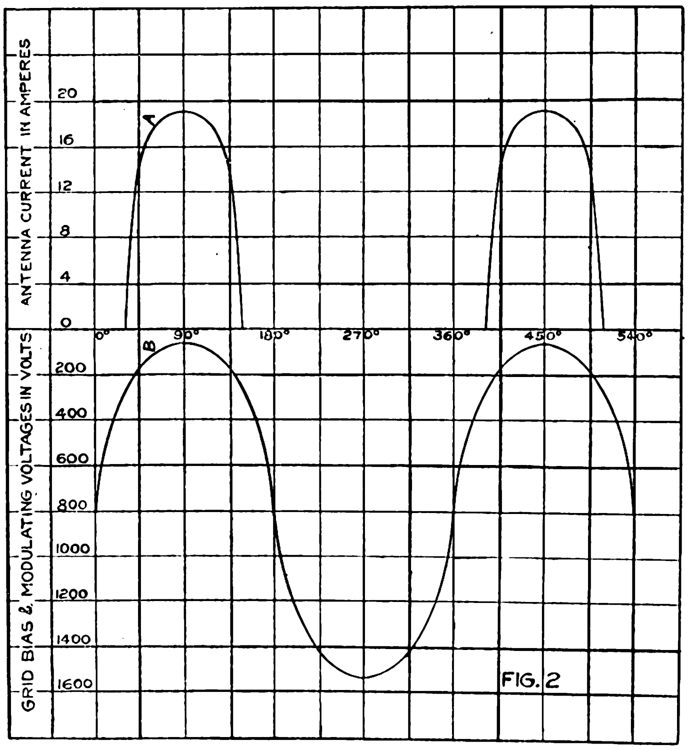

Fig. 2. — Forme d'onde de l'émetteur utilisé dans les expériences.

La figure 3 est une photographie des appareils d'émission.

Appareils de réception. — La différence entre les appareils de réception utilisés dans ces expériences-ci et ceux de Breit et Tuve réside dans les points suivants :

1) Emplacement différent des appareils. Les mesures furent effectuées sur le toit du bâtiment principal, alors qu'en été 1925 elles furent faites dans le petit bâtiment des expériences (Experiment Building).

2) Le dispositif extérieur de réception (receiving aerial) était un petit cadre de 2 pieds-carrés au lieu d'une antenne.

3) Dans l'amplificateur de puissance on utilisa des lampes de 7.5 watts à la place des lampes de 5 watts employées antérieurement. Dans chaque cas, on employait quatre lampes en parallèle.

4) Un oscillateur de 7.5 watts en couplage lâche avec le superhétérodyne remplaçait l'oscillateur 201 A employé antérieurement.

Résultats. — Les résultats obtenus sont résumés dans le tableau ci-dessous (p. 9).

On peut remarquer que le 19 août, non seulement l'onde réfléchie disparut, mais l'onde « de sol » (the ground wave) elle-même ne fut reçue qu'avec difficulté. Vers la même époque, l'intensité de la réception des signaux au « Naval Research Laboratory » était effectivement inférieure à la moyenne, certaines stations n'arrivant même pas du tout.

On apprit par des rapports, que les conditions anormales produisirent de nombreuses difficultés au circuit de haute fréquence San Francisco-Washington, tant de jour que de nuit. Durant les trois jours précédant et comprenant le 21, il y eut une grande perturbation dans le trafic avec Londres, et ce fut la première fois sur une période d'un mois que Londres ne reçut rien de la première émission.

Un rapport de Londres établit que pendant cette période les conditions de réception étaient mauvaises pour toutes les stations à ondes courtes à l'ouest de Londres, mais qu'elles étaient normales pour la réception des stations d'autres directions. Un autre rapport de Londres montre qu'on constata la même perturbation dans le réseau canadien (Canadian Beam). Toutefois les conditions ne semblaient pas affecter la réception à San Francisco de la station de fréquence 12,045 kilocycles de Washington. Le 19 août, Lakehurst, New-Jersey, ne put pas recevoir le message météorologique de 8 h. 15 du matin de Washington sur 4,015 kilocycles, et Pensacola, Floride, ne put capter ce message sur 8,030 kilocycles. L'impuissance de ces deux stations à recevoir ce message fut estimée anormale, et ne put être expliquée, le message météorologique ayant été correctement transmis.

Diverses personnes signifièrent au « Naval Research Laboratory » que pendant la perturbation, des messages (broadcast signals) dans la bande 550-1,500 kilocycles présentèrent aussi des anomalies.

Nous avons tenté de vérifier si cette condition anormale était effectivement reliée à une perturbation visible du soleil. On ne put obtenir aucun renseignement suffisamment précis car, bien qu'il y eût une tache extraordinaire dans le quadrant sud-ouest du soleil, elle avait été active plusieurs jours avant le 19 août.

Il semble toutefois que notre information soit bien exacte quant à montrer que la hauteur de la couche était de 50 milles environ avant le début de la perturbation, alors que cette hauteur augmenta jusqu'à 100 milles et plus après le retour aux conditions normales.

Les figures 4 et 5 donnent des reproductions de formes d'ondes caractéristiques observées pendant cette période. La première de celles-ci fut prise le 25 août et la seconde le 8 octobre. La figure 6 est une reproduction de la photographie du soleil le 19 août, mise gracieusement à notre disposition par S. B. Nicholson de l'Observatoire du Mont Wilson.

Détermination des hauteurs de la couche de Heaviside.

DATE	TEMPS	HAUTEURS EN MILES	ÉMETTEUR
1927	**h. m. h. m.**		
Août 15	2 00 — 2 15 soir	128 ou 66	Antenne
16	10 00 — 10 15 matin	70 ou 124	Antenne
16	2 00 — 2 15 soir	62	Antenne
17	10 00 — 10 15 matin	60 et peut-être 110	Antenne
17	2 00 — 2 15 soir	53	Antenne
19	. . . .	Pas d'onde réfléchie	Antenne
22	2 00 — 2 15 soir	112 ou 80	Antenne
23	10 00 — 10 15 matin	103	Antenne de Hertz
23	10 30 — 10 45 matin	104	Antenne
23	2 00 — 2 15 soir	104	Antenne
23	2 30 — 2 45 soir	124	Antenne de Hertz
24	10 00 — 10 15 matin	92	Antenne de Hertz (onde du sol plus intense que l'onde réfléchie)
25	10 15 — 10 30 matin	103	Antenne de Hertz (onde réfléchie plus intense que l'onde du sol)

Comparaison internationale d'étalons de fréquence,

par J. H. DELLINGER,
Chief of Radio Section, Bureau of Standards,
Washington D. C. (U. S. A.)

Nous résumons brièvement, dans cette note, les comparaisons internationales d'étalons de fréquence auxquelles le « Bureau of Standards » a participé pendant les quatre dernières années écoulées. La précision de ces comparaisons a augmenté progressivement, pendant cette période, répondant aux exigences croissantes de la pratique. Ces exigences nécessitent, de nos jours, des mesures de très haute précision.

On peut faire des comparaisons internationales d'une grandeur physique quelconque, en en expédiant des étalons successivement dans les laboratoires des divers pays. A une exception près, on a suivi cette méthode pour les comparaisons de fréquence. On crut, il y a quatre ans, pouvoir gagner en rigueur par une autre méthode, à savoir, la mesure de la fréquence dans divers laboratoires simultanément, d'ondes émises par des stations de puissance suffisante pour qu'elles puissent être entendues dans de nombreux pays. En 1924, un tel mode de comparaisons fut organisé par le « Bureau of Standards »; des mesures furent effectuées dans les laboratoires nationaux d'Angleterre, de France, d'Italie, d'Allemagne et des Etats-Unis. Les résultats de ces comparaisons, qui s'étendirent sur plusieurs semaines, ne furent que médiocrement satisfaisants. La précision des mesures fut inférieure à ce qu'on avait espéré. Bref, le résultat permit d'établir une concordance entre les divers pays à deux millièmes près. Mais l'examen des méthodes de mesure employées d'une part, certains résultats obtenus d'autre part, montraient nettement que les étalons des divers pays devaient être plus composables que ne semblaient l'indiquer les expériences mentionnées ci-dessus. Il est probable que les différences observées devaient être imputables aux modes opératoires employés dans ces comparaisons.

La conclusion fut qu'il y avait lieu de reprendre la méthode usuelle (transport d'étalons d'un laboratoire à l'autre, comme on fait à présent), méthode utilisée bien souvent pour d'autres grandeurs physiques. A cette époque, qui date déjà d'environ trois ans, on avait suffisamment étudié les possibilités d'emploi des oscillateurs et résonateurs piézo-électriques pour en dégager leurs avantages comme étalons portatifs. Les mesures faites à l'aide de dispositifs piézo-électriques semblaient offrir une supériorité sur la mesure simultanée de la fréquence d'ondes émises. La première tentative de ce genre fut une série de mesures, effectuées dans divers laboratoires nationaux par le Prof^r W. G. Cady, professeur à Wesleyan University, Middletown, Conn. Il emporta un certain nombre de résonateurs piézo-électriques (non pas d'oscillateurs); les résultats de ces mesures sont exposés dans son article « An international comparison of radio wave length standards by means of piezoelectric resonators » (1).

Une comparaison internationale d'étalons de longueurs d'onde radiotélégraphiques au moyen de résonateurs piézo-électriques.

En décembre 1925, le « Bureau of Standards » envoya en Europe un oscillateur piézo-électrique, d'abord au « National Physical Laboratory » en Angleterre, qui, à son tour, devait l'envoyer au général Ferrié en France, pour être transmis au Prof^r Vallauri en Italie, au « Physikalisch-Technische Reichsanstalt » en Allemagne, l'oscillateur devant faire alors retour au Bureau. En juillet 1926, un second oscillateur piézo-électrique suivit un chemin inverse, c'est-à-dire, en commençant par le « Physikalisch-Technische Reichsanstalt ». Il faut noter qu'on envoya des oscillateurs tout montés, et non les seules lames de quartz.

La lame de quartz est un étalon de fréquence dont les applications sont nombreuses, ce n'est cependant pas un étalon de constance absolue, ainsi que beaucoup l'avaient affirmé sans garanties suffisantes au moment de la découverte des propriétés de cette lame. De multiples facteurs introduisent des variations dans la fréquence de la force électromotrice produite par une lame vibrante de quartz, ainsi que dans la fréquence de résonance d'un piézorésonateur. Ces facteurs sont : la température, la méthode particulière et les détails de montage de la lame, les constantes des circuits employés, les méthodes de couplage entre le circuit dans lequel la lame de quartz est insérée et les circuits auxiliaires indispensables, ainsi que les circuits de mesure. Il est inexact qu'on puisse faire des comparaisons de haute précision en envoyant une lame de quartz d'un laboratoire à l'autre. L'effet des constantes du circuit peut faire varier de quelques pour mille la fréquence d'un oscillateur piézo-électrique; en conséquence, il faut envoyer un oscillateur tout monté, si l'on veut obtenir des résultats exacts.

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(1) *Proc. I. R. E.*, vol. XII, p. 805; December 1924.

Les mesures effectuées sur les deux oscillateurs piézo-électriques envoyés en Europe par le « Bureau of Standards », n'ont été que récemment achevées à cause du temps nécessité par l'envoi d'un laboratoire à l'autre. Les résultats sont très satisfaisants et montrent que l'usage d'oscillateurs piézo-électriques portatifs est une méthode de comparaisons internationales d'étalons de fréquence beaucoup plus sûre que la mesure simultanée de la fréquence des stations. Les différences entre les mesures, dans les divers laboratoires, atteignent au maximum huit dix-millièmes. Le résultat brutal indiquait une concordance de quelques dix-millièmes. Il restait de cette comparaison, un doute sur le point de savoir si les différences observées étaient imputables à des variations dans les oscillateurs piézo-électriques, utilisés comme termes de comparaison, ou à des différences que présenteraient actuellement les étalons des divers pays.

Cette méthode d'envoi d'oscillateurs piézo-électriques fut employée pour effectuer des comparaisons entre les étalons de fréquence des Etats-Unis, du Canada et du Japon. En février 1927, un oscillateur fut envoyé au Canada, où le « Radio Service of the Department of Marine » l'étalonna, puis fut renvoyé et réétalonné au « Bureau of Standards ». Les résultats furent du même ordre que ceux des comparaisons européennes, et l'administration canadienne estima qu'ils donnaient, pour la comparaison des étalons, autant de précision que le permet ce genre de mesures.

En mars 1927, le « Bureau of Standards » envoya un oscillateur piézo-électrique au Japon. Je viens de recevoir du Prof^r Yokoyama les résultats des mesures japonaises. La moyenne des valeurs japonaises ne diffère de celle du « Bureau of Standards » que de un dix-millième.

En février de cette année, un groupe de Sociétés commerciales américaines, savoir : la « General Electric C° », « American Telephone et Telegraph C° », « Westinghouse C° », « Radio Corporation of America », « General Radio C° », et deux départements gouvernementaux : la Marine et le « Department of Commerce », s'assemblèrent en conférence au « Bureau of Standards », et élaborèrent un programme dans le but de donner une base plus exacte aux mesures de fréquence. Chacun de ces organismes avait entrepris des recherches considérables à ce sujet, et il apparut que l'on progresserait plus rapidement en réunissant les ressources et l'expérience de tous. Pour mettre ce programme en œuvre la « General Electric C° », la « Westinghouse C° » et la Marine construisirent un certain nombre d'oscillateurs piézo-électriques perfectionnés. Ceux-ci réalisent un progrès par rapport à ceux que l'on utilisait antérieurement par le fait qu'ils possèdent un thermostat pour maintenir constante la température de la lame de quartz.

Comme j'allais en Europe cet été, pour le service du « Bureau of Standards », l'occasion se présentait d'utiliser l'un de ces appareils pour les comparaisons avec les étalons des laboratoires nationaux européens. Il était à espérer qu'en emportant dans ces laboratoires cet appareil perfectionné, on obtiendrait dans les comparaisons de fréquences, une précision qui surpasserait les précédentes. En effet, j'eus le plaisir d'effectuer des mesures de fréquences qui furent toutes satisfaisantes, au « National Physical Laboratory », en Angleterre, au laboratoire de la Télégraphie militaire du général Ferrié, à Paris, au laboratoire de la Marine italienne, à Livourne, en Italie, et au « Physikalische-Technische Reichsantalt », à Berlin. Je désire rendre hommage à la remarquable collaboration qui me fut offerte dans chacun de ces laboratoires par le D^r D. W. Dye au « National Physical Laboratory », le Prof^r R.

Jouaust, à Paris, le Prof^r G. Vallauri, à Livourne, et le D^r E. Giebe à la « Reichsanstalt ». Les mesures furent effectuées en juillet et août.

On mesura les fréquences de l'oscillateur piézo-électrique au « Bureau of Standards » avant mon départ des Etats-Unis et on les mesura à nouveau après mon retour. L'appareil renfermait deux lames de quartz dans une enceinte chauffée, avec régulateur de température, la lame de quartz étant maintenue à la température de 46°. Les appareils furent agencés de telle sorte que les voltages du filament et de la plaque restassent les mêmes. Les fréquences des deux lames de quartz étaient de 200 kilocycles environ.

Il était intéressant de constater que dans tous les laboratoires on utilise la même méthode générale pour les mesures de fréquence de haute précision. D'une manière très typique, on place dans le laboratoire trois générateurs distants de 10 pieds environ, l'un est l'oscillateur piézo-électrique à étalonner, l'autre est l'oscillateur auxiliaire ou hétérodyne, et le troisième est l'appareil-étalon par rapport auquel l'oscillateur piézo-électrique doit être étalonné. On couple à chacun de ces oscillateurs un circuit simple qui comprend un dispositif à réception téléphonique. La méthode de mesure est toujours une variante du procédé élémentaire qui consiste à écouter le son de battements entre le générateur auxiliaire et l'oscillateur piézo-électrique, à amener ce son à l'extinction par un procédé ou un autre et à amener à l'égalité de fréquence avec le générateur auxiliaire, l'étalon par rapport auquel on doit contrôler l'oscillateur piézo-électrique (ou encore déterminer la différence entre l'étalon et le générateur auxiliaire).

Les résultats justifièrent tous les espoirs. Les différences entre les mesures des divers laboratoires oscillent entre deux et cinq cent-millièmes, la moyenne des écarts de part et d'autre de la valeur moyenne étant trois cent-millièmes (the average of the departures from the mean being three parts in 100,000).

Cette concordance est, en effet, aussi bonne que le degré de précision des étalons nationaux. On peut en conclure que l'emploi d'un oscillateur piézo-électrique, muni d'un régulateur de température et dans lequel les courants qui parcourent les circuits sont toujours ramenés à la même valeur, permet d'accroître dix fois la précision obtenue par l'emploi de modèles plus simples d'oscillateurs piézo-électriques, du genre de ceux qui servirent aux comparaisons antérieures.

Ce n'est là cependant, en aucune sorte, la limite de la précision qu'on puisse atteindre. L'appareil avec lequel on fit les comparaisons était un de ceux avec lesquels n'avaient pas encore été accomplies les études complètes relatives, par exemple, à l'équilibre thermique, au temps nécessaire pour que les oscillations aient leur fréquence normale, et du coefficient de température de la lame de quartz. Il n'est pas douteux qu'avec le temps nous puissions tabler sur ces améliorations pour atteindre notre but, savoir une précision d'un cent-millième. Dans l'ensemble, cependant, j'estime établi que la précision avec laquelle les divers laboratoires nationaux mesurent les fréquences est plus que satisfaisante pour (in advance of) les exigences immédiates de la pratique radiotechnique. Ces exigences deviennent de plus en plus rigoureuses, et les laboratoires nationaux doivent donc poursuivre leur effort vers une précision toujours plus grande dans les mesures de fréquences.

* * *

The use of photoelectric cells associated with vacuum tubes for the solution of different problems in the measurement of time,

by G. FERRIÉ et R. JOUAUST,
Télégraphie militaire (Paris, France).

Measurements of intervals of time are of prime interest in numerous scientific problems concerning Radiotelegraphy, especially in frequency determination, which requires very high precision. Moreover the use of Radiotelegraphy for time signal transmissions, and the use of these latter in studies concerning wave propagation, origin of atmospherics (static), etc., enhance still further the importance of time measurement for specialists in Radiotelegraphy.

It is not possible to give here full details of

the methods and apparatus in use until now for the determination and preservation of time and for the measurement of intervals of time. We will limit ourselves to indicating some devices which we have developed by using potassium photoelectric cells associated with vacuum tubes and with amplifiers as used in the current technique of Radiotelegraphy (1).

Equipment of astronomical clocks. — Astronomical clocks, used in observatories for preserving the time determined by astronomical observations, have reached a very high degree of perfection. Their daily period is very regular and small, and is generally not greater than one hundredth of a second. Long time intervals can thus be measured with a very high accuracy.

On the other hand, when compared between themselves, the lengths of different seconds given by these clocks are seen to differ amongst themselves by notable quantities reaching sometimes a hundredth of a second. Such clocks have thus insufficient accuracy when used as intermediaries for measuring short time intervals of, say, a few seconds. The causes of these irregularities are :

The observatory clocks, being situated far from the point of observation, often even in cellars, are not directly seen by the astronomer, and, in order to permit the use of their indications, the following procedure is used :

The clock bears a device which closes an electric circuit once per second (most often every two seconds). The closing of this circuit passes a current which operates the pen of a chronograph or produces a « top » in a telephone. The closing device is constituted by a pallet borne by the pendulum and engaging with a toothed wheel. One can conceive that whatever the precautions taken in the cutting of the wheels their teeth are not identical.

Thus, the closing of the circuit does not occur at the precise instant when the pendulum is passing through its equilibrium position, and the difference depends upon the tooth which is in engagement with the pallet. The first idea coming to mind for eliminating this cause of error consists in seeking a means for observing the pendulum directly.

The principle of a device of this nature developed by us in 1923 for the Paris Obesvatory was as follows :

A mirror was fixed on the pendulum and, every time the pendulum was passing through its equilibrium position, the light rays coming from an electric lamp were reflected by the mirror and fell on the sensitive part of a potassium photoelectric cell. The photoelectric current, as amplified by 3 electrode vacuum tubes, was capable of operating recording apparatus.

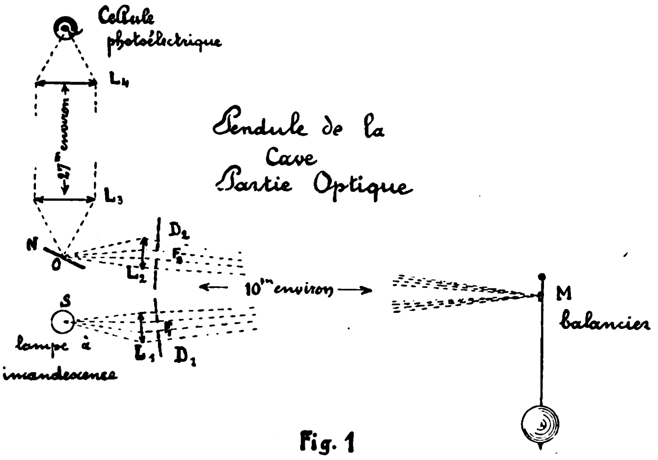

Fig. 1

In actual fact, the problem presented practical difficulties because the clocks of Paris Observatory are situated in cellars at a depth of about 27 meters. Figure 1 shows the solution which was adopted.

A plane mirror M was placed on the pendulum of the clock. The lens L_1 produced on this mirror the image of a powerful electric lamp S.

In a diaphragm D_1 placed before the lens, there was fine horizontal slit F_1, the rays coming from this slit came to reflection upon M.

The system was arranged in such a way that, when the pendulum was vertical, the rays reflected by M could pass through another horizontal slit made in the diaphragm D_2. They were received by a lens L_2 and gave at the point O on the mirror N an image of the lamp.

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(1) This work was begun in 1922 and the results were communicated successively to the meetings of the International Unions for Astronomy and for Geodesy and Geophysics in Rome in 1922, Madrid 1924, Cambridge 1926, and also published in the astronomical and radio magazines and in the *Comptes rendus de l'Académie des Sciences.*

The rays reflected by the mirror N, transformed into a parallel beam by the lens L_3, came up through a vertical well, the light of which was 27 meters, and were concentrated by means of the lens L_4 on the photoelectric cell.

It will be seen that on every passage of the pendulum through the vertical, the cell was illuminated for a very short time.

The photoelectric current was amplified by a two stage amplifier, the first stage of which consisted of a double grid vacuum tube and the second of a system of three ordinary tubes in parallel (fig. 2).

This amplified output, of about 4 to 5 milliamperes, was able to operate the pen of a recorder, or was passed through the primary of the input transformer of a low frequency amplifier and gave audible « tops » in the telephone.

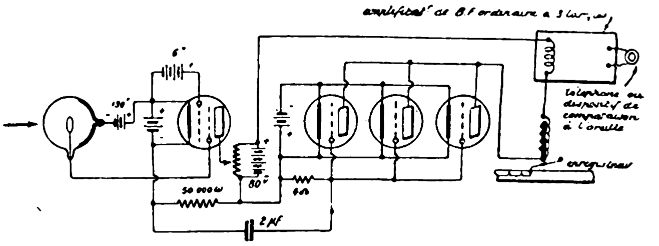
amplificateur de B.F ordinaire à 3 lampes
6v
130v
80v
50 000 ω
4 Ω
2 μF
téléphone ou dispositif de comparaison à l'oreille
enregistreur

Fig. 2. — Amplification for the clock of the cellar of Paris Observatory.

The recorder was a smoked-paper apparatus as used for sound ranging during the war; the deflecting system was an Abraham stylus oscillograph of the « électrométrique » type. The paper speed was 6 centimeters per second.

The oscillations of a 50 cycle tuning-fork were recorded on the same slip; this tuning-fork was electrically driven without contacts by means of 3 electrode vacuum tubes by the method simultaneously described in France by ABRAHAM, in England by ECCLES and JORDAN.

The slip thus obtained was examined by means of a dividing machine and showed also inequately of the different seconds. (Only seconds of the same sign were, of course, compared.) These irregularities were less than these shown in the closing of an electric circuit by the pendulum, but they were still important (about 5 to 6 parts in a thousand) and showed distinctly a periodicity of the order of a minute, a fact which indicates that the mechanism of the clock is the probable cause. Attention may be called to the fact that Mr. P. P. QUAYLE, of the Bureau of Standards, subsequently noted that the best chronometers of that institution also showed irregularities of several parts in a thousand for the lengths of their different seconds (1).

It is evidently in the mechanism of the clock that the cause of these irregularities must be sought.

It is known that any impulse which is not given to the pendulum at the precise instant when it is passing through its equilibrium position, has an influence on the period. This is what occurs with the teeth of the wheel trains of the clock which, not being exactly cut, do not operate all at the precise instant when the pendulum is passing through its equilibrium position. The result is a variation in the period of the pendulum.

The above considerations show that the clock pendulum is sometimes a little in advance, sometimes a little in arrear of the ideal pendulum which would be attained if no cause of disturbance existed and the period of which would be perfectly regular, the algebraical sum of these departures becoming zero after a time of about a minute.

Thus, if it is sought to determine the instant of a signal or a « top » recorded on the same strip as the clock beats by measuring on the strip the time interval which separates that signal or top from the nearest second, an error is made which equals the phase difference between the real pendulum and the ideal one at the moment of the second used as a reference point. If, on the contrary, the same instant is determined by measuring on the strip the time intervals which separate the signal or top from the 60 nearest seconds and by taking the average, the error is eliminated.

This method is long and it is furthermore difficult to apply to the comparison, not of a signal or « top » with the beats of a clock, but of a series of radio signals the record of which is sometimes incomplete, or of a series of beats of another clock to determine the period of the two clocks and their relative deviation. The accuracy of measurement thus in general leaves much to be desired.

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(1) *Journal of the Franklin Institute*, March 1927, p. 407.

The errors thus produced are shown for example in the publications of the « Bureau International de l'Heure » on the world-wide measurements of longitudes made in October-November 1926. In particular, important deviations are noted in the times of reception of the radio signals from ANNAPOLIS, between different determinations made on the same strip. These deviations are due to the fact that only a limited number of signals could be used for each of these determinations.

Self-maintained pendulum. — With a view to eliminating this cause of error, we have tried to produce a pendulum the period of which should be as constant as possible throughout the whole time over which its beats must be recorded to make the required comparisons, i. e. during a relatively short time of about 15 minutes maximum.

Instead of using a free pendulum as was subsequently done by certain experimentalists, for example Mr. BROWN, who used a geodetic pendulum (1), we produced in 1925 a pendulum maintained without any mechanical linkage by means of a light ray operating a potassium photoelectric cell. The principle of the apparatus is :

A pendulum of the type used in the little electrically driven clocks, working on the principle indicated a long time ago by CORNU, bears a magnet dipping horizontally into a coil. To maintain its motion, it is sufficient to send a current of appropriate sense through the coil every time the pendulum is passing through its equilibrium position. To obtain this current without the pendulum having to close any circuit by a material contact liable to disturb its period, a mirror is fixed on the pendulum rod; a light ray coming from a lamp is reflected by the mirror and illuminates the cell at the appropriate instant. The current thus produced in the cell is sent into a vacuum tube amplifier and thence through the coil. The duration of the impulse is only of 5 to 6 hundredths of a second and occurs exactly at the instant when the pendulum passes through the vertical.

One difficulty presented itself; the pendulum (beating half seconds) illuminates the cell every time it passes through its equilibrium position and thus as the photoelectric current is always in the same direction, the effect on the magnet of the pendulum is of contrary sence for two consecutive half-oscillations of the pendulum. The resultant is thus null. The following arrangement enabled us to overcome this difficulty :

Beside the free pendulum is set a little clock of the same period and type but provided with all its usual parts (contacts, clockwork, hands, dial) regulated as well as possible to synchronism, and maintaining itself electrically by the usual procedure. However, besides the winding used for autodriving, another concentric winding is traversed by the amplified current of photoelectric origin which already serves to maintain the free pendulum and which thus insures perfect synchronism between this clock and the free pendulum.

This supplementary clock permits the counting on its dial of the oscillations of its pendulum and so of those of the synchronous free pendulum. This clock controls also a device wich closes, every half-second of the same polarity (even or odd), a circuit controlling an electrically driven shutter set in the path of the reflected ray (1). In such conditions, the cell can receive the light only every two half-seconds and the free pendulum automatically maintains its motion without difficulties.

The complete device is as under (fig. 3 and 4) :

A straight filament lamp projects its light on a spherical mirror which gives an image of the filament on a diaphragm provided with a slit situated before the cell. The apparatus is regulated in such a way that the image is formed exactly on the slit when the pendulum is vertical. Very sudden starting of the photoelectric current is obtained in this way.

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(1) *Bulletin of the National Research Council*, vol. 11, part 12, No 56, 1926 November.

(1) In reality, to obtain good operation of the shutter, a slight displacement is established between the free pendulum and the counting-clock. This result is reached by regulating the clock to a period slightly different from that of the self-maintained pendulum. In such conditions a slight phase-displacement is known to be established between the synchronising pendulum and the synchronised one.

In series with the anode of the photoelectric cell is placed a 40 megohm resistance which is also connected between the filament and the grid of a 3 electrode vacuum tube. Any current produced by the cell causes a potential drop across the resistance and thus a change in the plate current of the first tube (1).

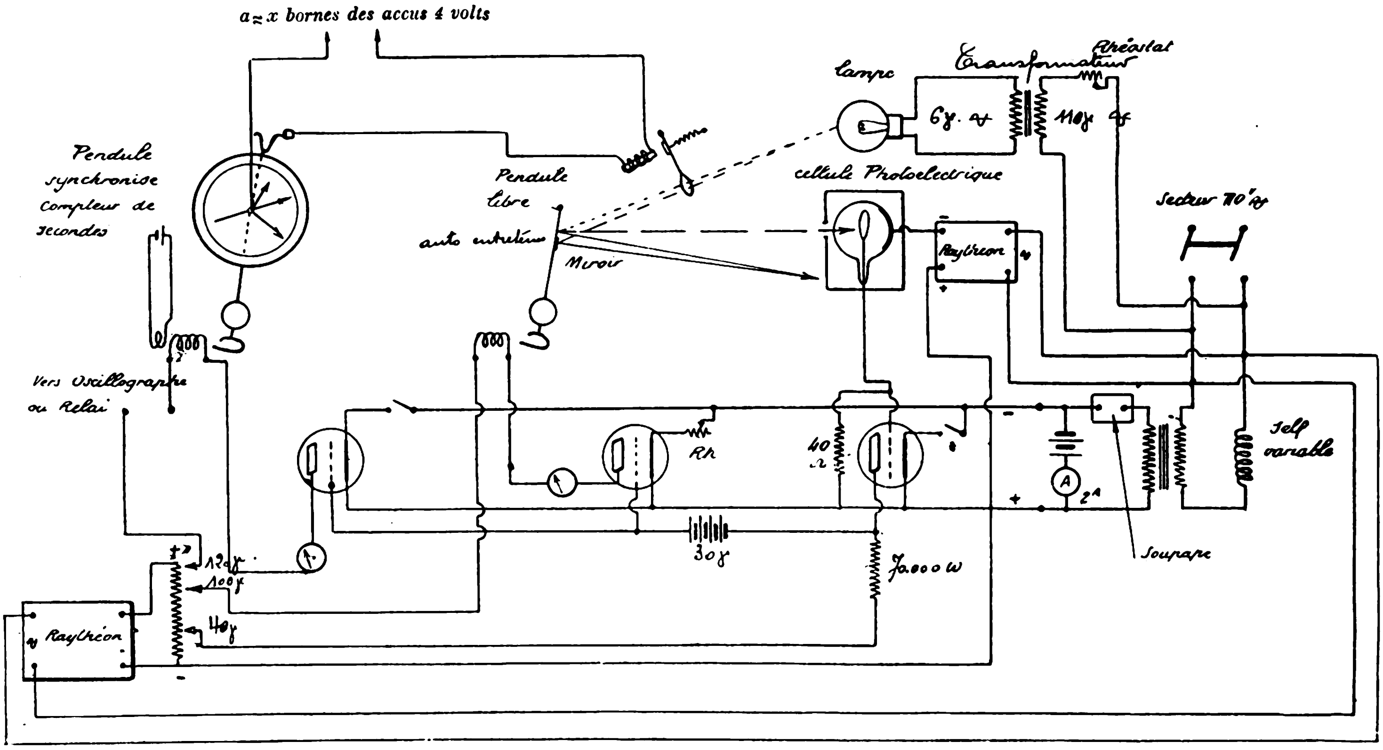

FIG. 4. — Self maintained photoelectric pendulum. (Set of Paris Observatory.)

This current change is amplified by means of an amplifier of the type called « direct current » (à courant continu) comprising two tubes in parallel.

The maintaining coil of the free pendulum is included in the plate circuit of one of these tubes; in that of the other tube are inserted the synchronising winding of the clock which controls the shutter and acts as a time-counter. The coils of the recorder or the windings of a relay controlling the transmitting apparatus of the radio station are also put in series with this winding.

To avoid the labour and inconvenience caused by the use and maintenance of batteries for the accelerating potential of the cell and for the feeding of the tube anodes, the alternating current lighting mains supplying Paris Observatory are used. The supply voltage, raised to a suitable value by a transformer, is rectified by means of a « raytheon », the current ripples being smoothed out by a filter. The cells are fed by one « raytheon », the tube anodes by another.

The tube filaments are also fed by alternating current from the mains, rectified by a tantalum-cell rectifier, a 4 volt storage battery being used as a buffer battery. The only battery remaining is the coupling battery between the two stages of tubes, a battery which does not give any output and does not require any care.

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(1) For amplifying the photoelectric current which is very weak, it is generally convenient to use a double-grid tube operating with low plate voltage, because of the absence of shock ionization of the residual gases, and thus of the tendency for discharge of the grid by positive ions. In the present case, with the strong illumination attained an ordinary 3 electrode vacuum tube is suitable and has been used because it has the advantage of avoiding the battery which would be necessary for polarising the internal grid of a double-grid tube.

The lamp illuminating the cell is also fed by the alternating current.

As the mains voltage is variable, the light-intensity of the lamp, and also the accelerating potential of the cell, are liable to change. Thence result possible changes in the impulses and thus in the amplitude of the self-maintained pendulum.

To remedy this inconvenience, the tube feeding the maintaining coil of the pendulum is run very dull. The potential changes across the grid in such conditions carry the plate current into the saturation region. The heating of the filament of the first amplifier tube has been so chosen that even the large fluctuations in the mains voltage still permit of the photoelectric current carrying the plate current of the tube into the saturation region (1).

In order to reduce to a minimum the changes in period due to changes in amplitude, the pendulum amplitude is very small, about 3°.

For making measurements with this system, we used the device already mentioned recording by ABRAHAM oscillograph on smoked-paper apparatus, a tuning-fork being recorded at the same time.

Here are the results of some of these measurements :

The system having been started at 8 a. m. the first record was made at 9 a. m. the second at 4 p. m.

10 consecutive tops were recorded each time.

Record at 9 a. m. — *Average* duration of pendulum period against tuning-fork period :

49,972 periods in a second.

Number of second.	Relative departure of the measured period from the mean period in ten-thousanths of a second.
1	— 4
2	+ 8
3	— 8
4	— 3
5	+ 4
6	— 1
7	+ 4
8	+ 1
9	— 1
10	0

Record at 4 p. m. — *Average* duration of the period against tuning-fork period :

49,98 periods (temperature had changed a little).

Number of second.	Displacement in ten-thousandths of a second.
1	— 2
2	+ 2
3	— 6
4	+ 6
5	+ 8
6	— 5
7	— 3
8	— 1
9	0
10	+ 1

These results show well that the self-maintained pendulum may be considered as giving oscillations constant in period within about 1 thousandth of a second. It may be that the accuracy is even greater, a thousandth being all that can be guaranteed with the recording procedure with smoked paper, because of the thickness of the line, the friction of the stylus, etc., whatever the precautions taken in the measurement of the strip, and *it must not be forgotten that the numbers given expressed in ten-thousandths are only there because they result from measurements made, but have in reality no physical significance.*

The pendulum rate has shown itself very regular. The beats of the master clock of the Observatory and those of the self-maintained pendulum were simultaneously recorded during two minutes at the beginning and the end of a quarter of an hour. The period of the self-maintained pendulum could thus be deduced as referred to the period of the master clock by measurements over 900 seconds.

A first measurement gave for the self-maintained pendulum a period too short by 7/100.000th. A second measurement made 3 hours later gave the value of 8/100.000th.

However, it must be remarked that these measurements were made at times of the day when the variations of supply voltage were relatively slight, which is not the case during the night and at dawn, and at this instant, in spite of the precautions taken, the same regularity cannot be assured. It would thus appear not to be possible *by means of the apparatus as now used*, fed from the lighting supply to establish a timekeeper preserving with certitude the 1/100th of a second after 24 hours operation. However, with an electric supply having only slight fluctuations, this result should be readily reached and even surpassed.

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(1) On the other hand the grid voltages of the direct current amplifier tubes are set to such values that their plate current is zero when the cell is not illuminated.

It will in any case be convenient, after using the self-maintained pendulum, to compare it as soon as possible with the master clock to determine its period.

The self-maintained pendulum, the period of which has been very accurately determined, is specially suited to the determination of the frequency of tuning-forks which are thereafter used for measurement frequency, using the Abraham « multivibrateur » or other devices.

Transmission of radio time signals. — To compare with precision the rythmic radio time signals, also called scientific signals, with the beats of the local astronomical clock, it is evidently necessary that these signals should be transmitted with perfect regularity, i. e. they should be equally spaced. It is unfortunately not so at present because the clocks controlling these signals have the irregularities, due to the wheel-trains, which were described and discussed above.

The use of the self-maintained pendulum has enabled this defect to be eliminated and transmission to be attained in which the interval between signals is constant within about one part in a thousand during trials made at the Paris Observatory. It is therefore sufficient to insert in the plate circuit of the second tube of the device of figure 4 the windings of BAUDOT relays which, from Paris Observatory, control the radio stations of the Eiffel Tower, ISSY and BORDEAUX-LAFAYETTE, to make these latter transmit signals which are controlled by the self-maintained pendulum. Certain new difficulties however, then present themselves : in the first instance the self-maintained pendulum must give 61 beats per minute. On this point there are no difficulties, it is merely a question of regulating the pendulum.

The first signal must furthermore be transmitted, as accurately as possible, at an exact minute. It is thus necessary that the self-maintained pendulum giving 61 beats in a second, should be maintained exactly to the time of the meantime-keeper clock of the Observatory, or rather that every group of 61 beats of the self-maintained pendulum should begin at the instant of the first second of every minute of the time-keeper, during the whole signal transmission.

This end is attained as follows. During the whole day, except at the instant of the beats, the self-maintained pendulum is synchronised by the mean-time-keeper clock, by substituting for its photoelectric maintaining current, the current furnished by the mean-time-keeper. This current must however be sent through the coil of the self-maintained pendulum only once per minute, at the instant of the first second of every minute of the time-keeper, as the time-keeper gives 60 beats per minute of mean-time while the self-maintained pendulum gives 61 beats in the same time interval.

Five minutes before the transmission of time signals, the synchronisation of the self-maintained pendulum by the mean-time-keeper is suppressed, and the photoelectric maintaining device is put in action. Then in the second preceding the minute, the circuit making the photoelectric current operate the BAUDOT relays controlling the radio transmissions is closed.

In such conditions, it has been possible to show that the first time signal was transmitted less than 1/10th of a second after the round minute of mean-time, what is sufficient for practical requirements. The exact time of this first signal is given later by the *Bulletin du Bureau International de l'Heure* for observers who require a higher accuracy.

It may be asked whether the different relays inserted between Paris Observatory and the transmitting stations, do not cause irregularities in the transmissions, because of the time-lag they introduce, a lag which might be variable. With a view to verifying this, we recorded at Paris Observatory over a period of five minutes on the same strip both the photoelectric currents of the self-maintained pendulum and the radio signals, and measured the delay between the two records, at the beginning and the end of the series of signals.

The results obtained are :

Beginning of record :

N° of the top.	Difference between the mean lag and the individual lag in ten thousandths of a second.
1	+ 3
2	— 4
3	+ 4
4	— 4
5	+ 1

End record :

N° of the top.	Difference between the mean lag and the individual lag in ten thousandths of a second.
1	+ 6
2	+ 1
3	— 6
4	— 4
5	+ 3

On the other hand, the two mean values differed by 1/10.000th only, i. e. a quantity within the limits of accuracy of measurement.

These results thus shows that the device considered will permit the transmission of signals the interval of which shall be constant within about one thousandth of a second. The device will be put into use at Paris Observatory at an early date.

Determination of time. — For the sake of completeness we may refer to another use of photoelectric cells in the astronomy of position, and thus in time measurements : the determination of the instant of transit of stars across the meridian. This use was the subject of a paper in the *Proceedings of the Institution of Radio Engineers,* vol. XIII, n° 4, August 1925, p. 461.

Study of gravity. — Mention may also be made of the ease of making a device similar to that of the cellar clock of Paris Observatory (fig. 1 and 2), but very much simplified, for the measurement of the period of a free pendulum set up for determining the intensity of gravity (fig. 5).

We have to mention that different experimentalists have also tried to use photoelectric cells combined with vacuum tubes for ends similar to those we were trying to attain from 1922, and which we attained in 1925.

QUAYLE, of the Bureau of Standards, for the study of tuning-forks, made use as a time indicator of a free pendulum carrying a mirror which reflects a light ray operating a photoelectric cell (1).

GOULD, of the Bureau of Standards, has also recently tried to control time signal transmission by a free pendulum operating a cell sensitive to light.

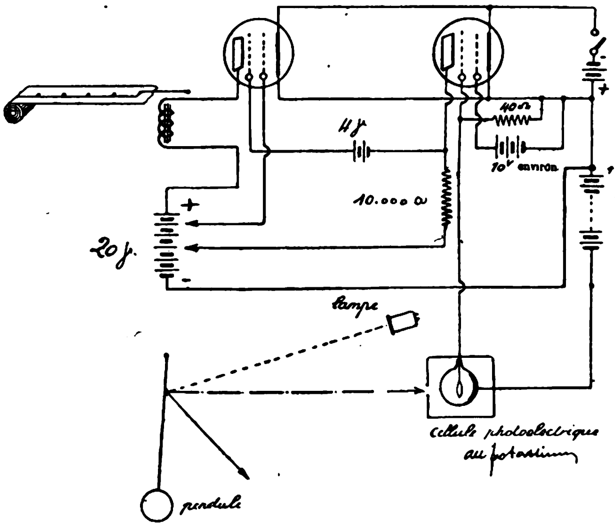

FIG. 5. — Photoelectric recording set for geodesic pendulum.

In the *Bell Company* laboratories, a pendulum self-maintained by means of a light ray was also produced in 1926. The method of operation was a little different from that which we have described. The photoelectric current caused the closing of a relay and this closed a battery circuit on the maintaining coil of the clock.

The photoelectric effect thus only took the place of the pallet and toothed wheel assembly ordinarily used.

* * *

Mesure précise de la fréquence,

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Lorsque nous parlons de la fréquence d'une onde ou, en fait lorsque nous utilisons le mot fréquence dans une quelconque de ses significations, nous voulons en général désigner le nombre de fois qu'un phénomène périodique se produit dans un intervalle de temps donné. En

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(1) *Journal of the Franklin Institute,* 1927 March, p. 407.

conséquence, la fréquence d'un phénomène périodique est l'inverse de l'intervalle de temps nécessaire au parcours d'un cycle, pourvu que la durée d'un cycle quelconque soit constante, ou, en d'autres termes, que le phénomène ait une fréquence constante. Puisque la fréquence peut être exprimée complètement en fonction du temps, il n'est ni nécessaire ni désirable d'avoir un étalon fondamental de fréquence différent de l'étalon de temps agréé.

L'étalon de temps actuel est le jour sidéral ou temps mis par la terre pour faire une révolution complète dans l'espace. Une succession de jours sidéraux constitue donc un phénomène périodique dont une période quelconque est, par définition, égale à toute autre période. En d'autres termes, le phénomène possède la caractéristique essentielle requise pour constituer un étalon de fréquence. Le jour solaire moyen, en fonction duquel on exprime toutes les mesures, doit être considéré comme un étalon pratique dont la détermination est faite en fonction du jour sidéral.

Il y a une seule différence entre un étalon de temps ou de fréquence et les étalons de la plupart des autres grandeurs. On peut par exemple donner à l'étalon de masse et à celui de longueur une existence matérielle, et les préserver de telle sorte que leur identité se conserve pendant de longs intervalles de temps. A l'opposé d'une masse et d'une longueur, une durée ne se prête pas, d'elle-même, à être conservée, ni à recevoir promptement une existence matérielle, qui puisse être employée, en vertu de sa grande stabilité, comme étalon fondamental. A l'heure actuelle, toutes les mesures précises de temps doivent donc se ramener à la mesure de la période de la révolution terrestre.

Si l'on est, une fois pour toutes, arrivé à la conclusion que l'étalon fondamental de temps est, *ex officio*, l'étalon fondamental de fréquence, il ne reste plus qu'à comparer la période d'un phénomène périodique quelconque qui doit être mesuré, à la période de la révolution terrestre.

Si un phénomène périodique de période inconnue ne pouvait être aisément comparé qu'à l'intervalle de temps défini par la rotation de la terre, il serait nécessaire que la période du phénomène inconnu demeurât invariable pendant toute la durée d'une révolution terrestre. Dans ce cas, la mesure consisterait à compter le nombre de fois que la variation inconnue se répète pendant cette durée. Toutefois, grâce à la commodité des moyens qui permettent de défininr exactement des intervalles de temps plus courts que le jour sidéral, avec la précision désirée, il n'est pas nécessaire d'entretenir le phénomène à mesurer pendant un long intervalle de temps. Quand nous apercevons qu'un moyen quelconque utilisé pour évaluer de courts intervalles de temps, présente lui-même des variations périodiques de période invariable nous pouvons nous considérer comme étant en présence d'un étalon secondaire de fréquence aussi bien que d'un étalon secondaire de temps. Ainsi, les horloges « Riefler » installées au « National Bureau of Standards » et au « United States Naval Observatory » deviennent nos étalons de fréquence les plus pratiques (1). En ce qui concerne les oscillations de leurs pendules, on peut dire que leur fréquence par rapport à la seconde correspond à une unité.

Un étalon secondaire tel qu'un pendule (high grade seconds) ne convient pas actuellement à la détermination de la fréquence des variations qui sont intéressantes dans les communications électriques, tant à cause du rapport excessivement grand entre les fréquences des événements à comparer qu'en raison du fait que, dans sa forme actuelle, le mouvement du pendule d'horloge ne convient pas bien à la comparaison des variations électriques. Il est donc désirable, comme premier pas vers la mesure précise des fréquences d'ondes électriques, de trouver un étalon de comparaison convenable. Les conditions requises par un tel étalon sont les suivantes : sa période doit être du même ordre de grandeur que les périodes à mesurer, la forme de la variation qu'il commande doit se prêter à une comparaison aisée avec les variations à mesurer, et, enfin, sa période doit être suffisamment constante, de telle sorte que le nombre total d'oscillations qu'il exécute en un laps de temps dont la durée soit mesurable avec une haute précision, puisse être pris comme fréquence de la variation sur de plus courts intervalles de temps. Cette dernière condition fait apparaître à nouveau la similitude de nature entre les étalons secondaires de fréquence et les étalons secondaires de temps. Pour la facilité, il sera également possible de régler la période de l'étalon de fréquence à une certaine valeur fixée dans des limites telles que l'erreur puisse être négligée dans la majorité des cas.

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(1) Ceci, bien entendu, ne s'applique qu'aux Etats-Unis; mais on peut faire de même avec n'importe quel étalon de temps national.

Montage initial. — Un étalon secondaire ayant les caractéristiques énumérées ci-dessus a été décrit il y a plusieurs années par les auteurs du présent mémoire en collaboration avec M. N. H. Ricker.

Cet étalon consiste en un diapason de fréquence 100 dont les vibrations sont entretenues par un amplificateur relié au diapason par couplage électromagnétique. On a fait en sorte d'obtenir de la partie électrique du système oscillant ainsi établi, un courant alternatif sinusoïdal dont la fréquence soit constante avec un haut degré de précision. Les étalons de ce type général deviennent d'un emploi très fréquent, comme on le verra en consultant la bibliographie. Un court examen de l'état actuel de cet étalon de fréquence 100 est intéressant non seulement à cause du progrès réalisé dans sa construction depuis que le rapport initial fut présenté, mais aussi parce que l'expérience acquise par son emploi a été précieuse dans la construction d'étalons encore plus utiles.

Le diapason décrit dans le mémoire déjà cité, fonctionna d'une manière continue depuis avril 1923 jusqu'en mai 1927, sauf pendant quatre intervalles formant un total de trois jours. Parmi ces interruptions, deux furent dues à des causes accidentelles et deux provoquées dans le but d'introduire de légères modifications au système. Pendant tout ce laps de temps, la température du diapason entretenue par un bain d'eau fonctionnant comme thermostat, était maintenue approximativement à la valeur pour laquelle la fréquence est de 100. Comme le coefficient de température de la fréquence est de 0.0109 pour cent par degré centigrade pour le diapason visé, il est nécessaire de faire en sorte que les écarts de température ne dépassent pas 0.010° C afin que la période puisse être exacte à un millionième près. On a trouvé que le coefficient de température, au lieu d'être une cause d'erreur a fourni un excellent moyen de régulation de la fréquence, en rendant possibles de petites variations dont les valeurs puissent être exactement déterminées à l'avance. Pendant les quatre années de fonctionnement, la variation dans la fréquence du système complet, due à la fois au vieillissement du diapason et à toutes les autres causes, et déterminée par la variation de la température — indiquée par un thermomètre de Beekman — à laquelle la fréquence est exactement 100, a été inférieure à 0.004 pour cent.

Le moteur synchrone décrit dans le mémoire antérieur a été remplacé par un type perfectionné représenté à la figure 1. Le moteur initial était couplé à l'horloge à l'aide d'un engrenage réducteur commandant un commutateur, dont les impulsions étaient communiquées à un mécanisme d'horlogerie pourvu d'un équipement électromagnétique. Le moteur actuel est engrené directement sur le mouvement d'horlogerie, ce qui élimine complètement les erreurs dues au mécanisme. En dépit de la méfiance générale à l'égard du moteur synchrone, on a pu constater que les deux moteurs employés avaient fonctionné d'une façon entièrement satisfaisante pendant de longues durées. En réalité les seuls cas d'arrêts de ces moteurs en service ont été dûs à des absorptions de puissance par quelque partie du système.

Lors des essais de cet étalon de fréquence, on a trouvé qu'il constituait un garde-temps bien plus sûr que l'horloge à pendule entretenu électriquement qu'on a employé pour obtenir les données déjà publiées. On se dispensa donc de l'horloge à pendule et toutes les mesures de la période du diapason sont faites maintenant par comparaison avec le jour solaire moyen défini par les signaux horaires radiotélégraphiques envoyés par le « U. S. Naval Observatory ». A l'aide de son aiguille des secondes, qui est concentrique avec l'aiguille des heures et des minutes et qui tourne uniformément, l'erreur quotidienne dans l'horloge contrôlée par le diapason peut être déterminée à 0,2 de seconde près, ressortant ainsi à trois millionièmes environ pour la durée du jour. Pour une plus haute précision, on a placé dans le train d'engrenages, entre le moteur et l'horloge, un axe faisant un tour par seconde et fermant un contact à l'aide d'une came. Les pulsations de ce contact peuvent être enregistrées simultanément avec les radiosignaux horaires sur un ruban de papier à l'aide d'un récepteur à siphon à deux éléments. A partir de l'enregistrement ainsi obtenu, l'erreur de l'horloge peut être déterminée à 0,01 seconde, augmentant d'environ vingt fois l'exactitude de la détermination.

Les nombres représentés par le diagramme de la figure 2 sont caractéristiques de la perfection de ce système. Sur ce graphique, l'erreur dans la lecture de l'horloge est portée en fonction des jours. La période est déduite de l'allure de la courbe, lorsque celle-ci est droite, la période est constante, quand elle est horizontale, la fréquence est exactement de 100. L'attention doit être attirée non seulement sur l'invariabilité de la fréquence, qui ne s'est jamais éloignée de plus de 0,0007 pour cent de la valeur moyenne pendant toute la durée de l'enregistrement, mais encore sur la précision avec laquelle le système était réglé à la valeur absolue désirée.

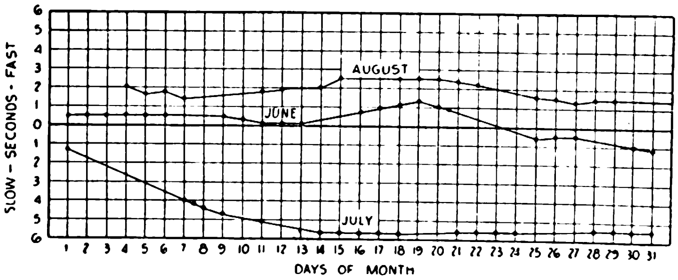

FIG. 2. — Erreur quotidienne de l'horloge commandée par l'étalon de fréquence à 100 cycles.

Fréquence moyenne pour une période de trois mois : 99.99991 cycles.

Ecart moyen de la fréquence moyenne : 0.00016 cycles.

Ecart maximum de la fréquence moyenne : 0.00067 cycles.

Dans le Mémoire antérieur, nous avons exposé une méthode pour la comparaison de hautes fréquences à l'aide de l'étalon à 100 périodes. Cette méthode implique l'emploi de dispositifs producteurs d'harmoniques et de réseaux de sélection des harmoniques. On peut, par la production et la sélection répétées d'harmoniques, obtenir n'importe quel multiple de la fréquence fondamentale jusqu'à une limite qui dépend du soin apporté à l'opération. Théoriquement, le processus peut être poursuivi indéfiniment, mais pratiquement, il se présente des difficultés à dépasser deux ou trois rangs d'harmoniques. Ces difficultés proviennent de petites irrégularités dans la fréquence ou dans l'amplitude du courant dans la fondamentale du système, qui à leur tour sont causes des irrégularités dans la fréquence de l'onde finale. Tandis que ces irrégularités peuvent être entièrement négligées dans l'onde dont elles proviennent, elles sont fortement amplifiées dans les étages suivants.

En procédant avec attention on peut obtenir, avec le courant étalon à 100 périodes, un courant de fréquence 100.000 ou même davantage. En utilisant un oscillographe cathodique, on peut régler un oscillateur indépendant de telle sorte que sa fréquence soit un multiple exact de la fréquence du courant ainsi obtenu. Ce processus peut être répété aux étages successsifs jusqu'à la limite d'emploi de l'oscillographe.

Le système contient donc trois éléments essentiels : le diapason entretenu constitue une source de courant alternatif, dont la fréquence est de 100, avec un très haut degré de précision ; le moteur synchrone et le train d'engrenage de l'horloge constituent un système de réduction de la fréquence (rate) présentant un rapport d'environ dix millions à un. Le dispositif producteur d'harmoniques et l'oscillographe cathodique peuvent être réglés de façon à augmenter la fréquence dans le rapport de plusieurs milliers à un.

On peut donc déterminer, à l'aide de ce système, la fréquence d'une onde électrique, en utilisant le jour solaire moyen comme étalon, quand le rapport entre les fréquences des deux phénomènes n'excède pas 10^{11}.

Equipement actuel. — Au moment où la construction de l'étalon de 100 périodes fut entreprise, l'étendue des fréquences à mesurer avec grande précision ne dépassait pas quelques centaines de mille périodes par seconde. Pour ce travail, la fréquence choisie comme étalon secondaire était tout à fait satisfaisante. Depuis lors, cependant, on a prêté de plus en plus l'attention aux fréquences plus élevées, si bien qu'il est devenu nécessaire d'effectuer des mesures avec un très haut degré de précision sur des fréquences de plusieurs millions. Toute augmentation dans la fréquence de l'étalon de référence s'obtient aux dépens d'une augmentation dans le rapport entre sa fréquence et le jour solaire moyen. Cette circonstance ne peut être admise cependant sans objection formelle puisque la comparaison de ces deux périodes peut être faite au moyen d'appareils réglés d'une manière permanente et expressément construits pour ce rapport fixe, alors que les comparaisons entre les périodes inconnues et celle de l'étalon de référence doivent être faites au moyen d'appareils soigneusement réglés, capables de fonctionner sur une gamme étendue. Mais il y a plus : on a trouvé que la fréquence d'un courant donné peut être commandée par un courant de fréquence plus élevée avec une stabilité plus grande qu'avec un courant de fréquence moindre. A cause de ces divers facteurs, il est nécessaire de posséder un étalon du type général décrit ci-dessus mais ayant une fréquence fondamentale bien plus élevée. On a construit un tel étalon et on a obtenu quelques résultats préliminaires quant à la stabilité de sa période.

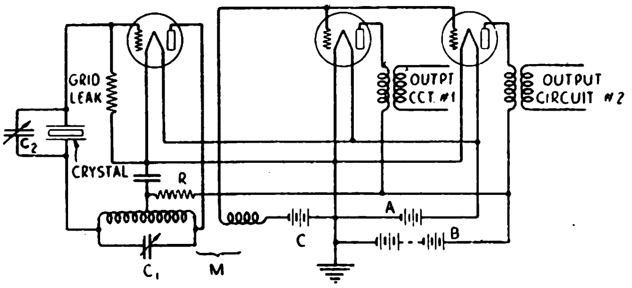

FIG. 3. — Circuit de l'oscillateur pour l'étalon de haute fréquence.

Le générateur de haute fréquence de cet étalon est un oscillateur commandé par un cristal de quartz de fréquence 50.000 muni de dispositifs qui présentent une grande stabilité. La figure 3 représente le circuit de cet oscillateur. Il est du type Hartley, ne différant du type habituel que par un cristal piézo-électrique de résonance connecté au circuit de grille. Ce type de circuit est utilisé parce qu'il est commode, à l'aide de réglages de circuits, de vérifier la phase de la réaction régénératrice (*regenerative feedback*) pour que la stabilité maximum soit obtenue. Comme il constitue un circuit oscillant efficace, il permet aussi d'employer le cristal en couplage lâche avec le circuit électrique, permettant donc de bénéficier du très faible décrément du cristal.

Le cristal est réglé (en lapping) de telle sorte que la fréquence du circuit qu'il commande soit de 50.000 exactement à une température ambiante donnée. Les légères corrections subséquentes peuvent être faites à l'aide d'un petit condensateur variable en parallèle avec le cristal.

L'oscillateur commandé par un cristal est couplé à deux circuits par des lampes ayant leurs grilles en parallèle, en couplage lâche avec la bobine d'accord. De cette façon, on peut obtenir un courant considérable de fréquence 50,000 sans que des circuits extérieurs ne puissent réagir sur l'oscillateur.

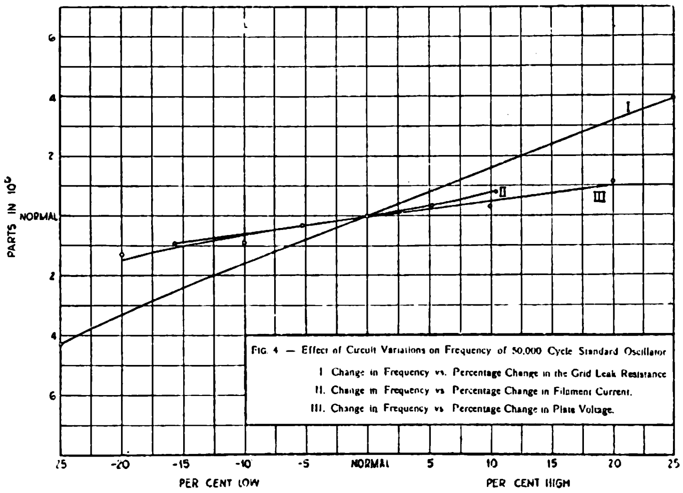

FIG. 4 — Effect of Circuit Variations on Frequency of 50,000 Cycle Standard Oscillator

I. Change in Frequency vs. Percentage Change in the Grid Leak Resistance

II. Change in Frequency vs. Percentage Change in Filament Current.

III. Change in Frequency vs. Percentage Change in Plate Voltage.

Les trois courbes de la figure 4 mettent en évidence la stabilité de la fréquence de l'oscillateur sous l'influence de variations dans le courant de filament, dans la tension de plaque et dans la résistance de grille. On porte l'écart de la fréquence normale en millionièmes en fonction de l'écart en pour cent du courant normal de filament de la tension normale de plaque et de la résistance normale.

La variation due à la résistance de grille est la plus grande, mais grâce à l'emploi d'un type spécial de résistance, prévu pour ne pas varier de plus de 0.1 pour cent pendant le fonctionnement, les variations dans la fréquence, dues à cette cause, sont négligeables.

La variation due à la tension des batteries est le facteur le plus sérieux, étant de même sens pour les deux batteries. Ces tensions sont vérifiées actuellement avec une approximation de plus ou moins 2 pour cent, on peut donc s'attendre à des variations subséquentes dans la fréquence, d'environ trois dix-millionièmes. Bien entendu, comme on l'a fait dans d'autre cas, il est possible de compenser les variations du circuit dues à des variations de tension, de façon que toute tendance de la fréquence à varier dans un certain sens soit contrebalancée par une tendance égale et opposée.

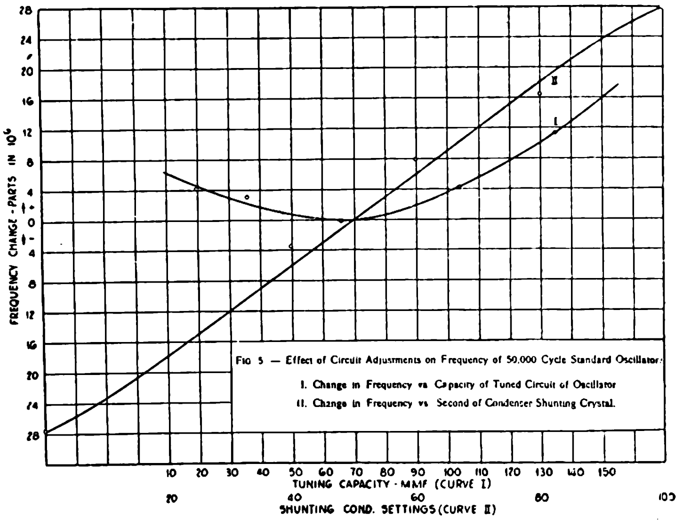

Fig 5 — Effect of Circuit Adjustments on Frequency of 50,000 Cycle Standard Oscillator:

I. Change in Frequency vs Capacity of Tuned Circuit of Oscillator

II. Change in Frequency vs Second of Condenser Shunting Crystal.

Les courbes de la figure 5 représentent les variations dans la fréquence de l'oscillateur pour des variations dans les capacités C_1 et C_2. Ces courbes représentent des variations dans la fréquence en millionièmes et sont tracées, dans le cas du condensateur C_2, en fonction des lectures du cadran et, dans le cas du condensateur C_1, en fonction de la capacité en micro-microfarads. Grâce au fait que le réglage obtenu par le condensateur C_2 est linéaire, on peut produire dans la fréquence agissante des légères variations déterminées à l'avance. Le réglage convenable pour le condensateur C_2 est très voisin de la valeur pour laquelle la variation de la fréquence avec la capacité est nulle. Quand cette condition est réalisée, les petites variations dans le circuit électrique causées par des variations de température, ont un effet entièrement négligeable sur la fréquence.

La figure 6 représente le cristal de quartz monté. Il vibre dans le sens de sa longueur et il est supporté par une petite pièce de feutre au centre, de façon à lui permettre de vibrer librement. Il est maintenu en place et espacé des plaques par des fils de soie. Le décrément logarithmique de ce cristal monté, l'ensemble étant mesuré comme un circuit électrique, est d'environ 0.00012. Les plaques conductrices sont rendues très rigides de façon à maintenir constant l'espacement, car une variation de $0^{mm}01$ seulement dans cet espacement entraînerait une erreur appréciable dans la fréquence. Le coefficient de température de la fréquence est d'environ 0.00038 pour cent par degré centigrade, impliquant un réglage de la température constant à 0.0250° C près, pour que les variations dues à la température restent de l'ordre de un dix-millionième. Des mesures de l'influence de la pression atmosphérique, dans le domaine des pressions allant de 760 millimètres aux pressions très basses, montrent que la fréquence est approximativement une fonction linéaire de la pression. Pour le cristal particulier sur lequel portèrent les mesures, on trouva qu'une augmentation de pression de 140 millimètres produit une diminution dans la fréquence de 0.001 pour cent.

La figure 7 représente le thermostat actuellement employé pour régler la température du cristal. Il consiste en un cylindre d'acier à parois creuses remplies de mercure; ce mercure, lorsqu'il s'échauffe, s'épanche par un tube capillaire comme dans un thermomètre. Pour une certaine température, il entre en contact avec un fil de tungstène taillé en pointe (non représenté), ce contact déclanche un mécanisme qui agit pour modifier le chauffage, lequel est fourni par une bobine de résistance enroulée autour du cylindre d'acier. Le cristal est monté à l'intérieur du cylindre et tout l'appareil est enfermé dans une boîte en bois de balsamier pour être thermiquement isolée. A l'aide de ce dispositif, la température en un point intérieur du cylindre peut être maintenue à une valeur prescrite pendant de longues périodes.

Il est intéressant d'observer que la température du cristal pendant le fonctionnement n'est pas exactement celle de l'espace environnant, parce qu'il est le siège d'une perte d'énergie. Pour ce motif, et puisque la perte croît au moins comme le carré de la tension de plaque appliquée, cette tension est maintenue à une valeur faible. La résistance R dans le circuit de la figure 3 répond à cette condition.

Le circuit de la lampe de l'oscillateur, avec le cristal et le régulateur de température, est représenté en photographie par la figure 8. Tout ce circuit est monté dans un compartiment clos dont la température est elle-même commandée par un régulateur du type commercial.

Un élément important de ce nouvel étalon de fréquence est le générateur de fréquences sous-multiples qu'on emploie au préalable pour comparer à l'étalon de temps la période de l'oscillateur à cristal. Ce générateur de fréquence sous-multiples consiste en un oscillateur à basse fréquence, un générateur d'harmoniques pour obtenir un harmonique de la basse fréquence qui corresponde à la haute fréquence qui commande, et un modulateur qui donne un courant continu dont l'amplitude est fonction de la différence de phase entre le courant régulateur de haute fréquence et l'harmonique du courant réglé de basse fréquence. L'action du courant continu sur un dispositif de réglage (control element) dans l'oscillateur à basse fréquence maintient la fréquence à une valeur telle que le courant du générateur d'harmonique ait exactement la même fréquence que celui de l'oscillateur étalon. La figure 9 représente la disposition des circuits. Les lampes, à partir de la gauche, sont associées (associated) respectivement au modulateur, à l'oscillateur, au générateur d'harmonique et à un amplificateur de débit.

Le dispositif de réglage consiste en un noyau de substance magnétique possédant deux enroulements, l'un placé dans le circuit oscillant lui-même et l'autre dans le circuit de plaque de la lampe modulatrice. Cette bobine est montée de telle sorte qu'une variation dans l'intensité du courant continu (émis) du modulateur produit une variation dans l'inductance de l'enroulement du circuit oscillant grâce à la saturation magnétique du noyau.

Pour la mesure, l'oscillateur à basse fréquence est réglé de manière que la fréquence soit exactement un sous-multiple de la fréquence régulatrice (of the control frequency) quand le courant continu dans la bobine de réglage a une certaine valeur moyenne. Si une cause quelconque tend à modifier la basse fréquence, il en résulte alors un changement de phase entre l'harmonique et le courant régulateur qui produit instantanément une variation dans le courant continu de la bobine de réglage, variation qui a pour effet de s'opposer à cette tendance. Il en résulte qu'en dépit de grandes variations dans le circuit de basse fréquence, la fréquence est maintenue exactement à un sous-multiple de la haute fréquence; la seule variation consiste en un léger changement de phase (shift in phase) par rapport au courant régulateur.

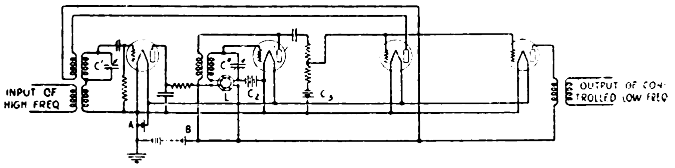

Fig. 9. — Générateur de fréquence sous-multiple contrôlée.

Avec un circuit fonctionnant de la sorte, il est possible de régler (to control) un courant de telle manière que sa fréquence soit un multiple exact de celle d'un autre courant quelconque, même quand ce dernier a une fréquence beaucoup plus élevée. On peut obtenir immédiatement un rapport de 50 à 1 sans excès de précautions. Il serait donc parfaitement possible d'obtenir, par étages successifs, un courant ayant une fréquence qui fût exactement, un cinq-centième de la fréquence de l'étalon à 50,000 périodes, soit 100. Ce courant pourrait alors être utilisé pour actionner le moteur synchrone de l'horloge décrit antérieurement. Aujourd'hui on a trouvé, avantageux de produire un courant ayant une fréquence égale à un dixième de celle de l'étalon et d'employer ce courant pour actionner un moteur synchrone à 5,000 périodes.

Le moteur synchrone à haute fréquence agit sur le débit d'une simple lampe de 5 watts. Le rotor est monté sur un arbre d'acier vertical tournant sur une simple bille d'acier noyée dans l'huile. Le moteur est monté sous une cloche et fonctionne dans un vide partiel afin de diminuer les pertes par frottement et de conserver les paliers propres. Quand on isole le moteur de la source de puissance, et s'il tourne à sa vitesse maximum, la rotation perdure pendant plus d'une demi-heure.

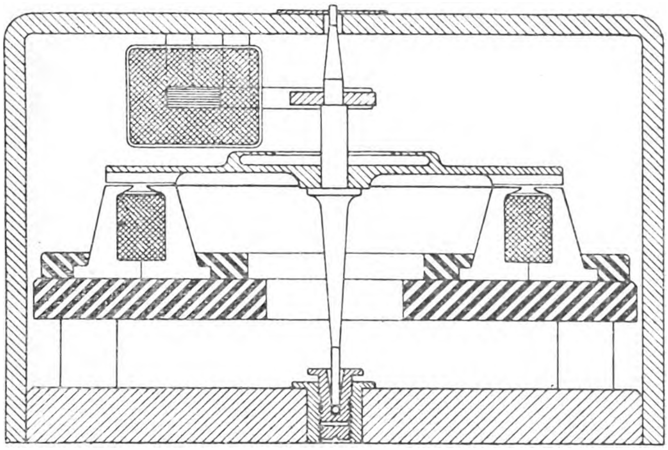

Fig. 10. — Coupe du moteur synchrone à 5,000 cycles.

Le rotor, est un disque plat sur le bord duquel sont creusés, d'un côté cent dents servant de pôle, et qui possède une cavité annulaire dans la partie supérieure pour abriter le mercure servant à éviter les oscillations. Le stator consiste en 100 pièces de ferro-silicium taillées en U, disposées suivant des plans passant par l'axe, et régulièrement espacés autour de cet axe. Ces pièces forment un anneau de même diamètre extérieur que le rotor, possédant un espace annulaire dans lequel se trouve une bobine pour aimanter tous les pôles en parallèle. Une seule de ces pièces taillées en U et une seule dent du rotor forment ensemble un circuit magnétique autour de la bobine. On emploie, par ce mode de construction, le minimum de matériel magnétique et il est possible de laminer soigneusement chaque partie. On peut enrouler des bobines pour compenser l'impédance de n'importe quelle lampe à utiliser et on peut remplacer aisément ces bobines. L'espace entre le rotor et le stator peut être modifié dans chaque cas particulier en réglant le support inférieur. Les figures 10 et 11 représentent une coupe et une photographie du moteur monté.

La bobine du moteur est connectée au circuit de plaque de la lampe employée pour l'entraîner de telle sorte que le courant continu et le courant alternatif circulent à la fois dans l'enroulement. Cela produit 5,000 pulsations magnétiques par seconde au lieu de 10,000 comme ce serait le cas si l'on n'employait que le courant alternatif. Ainsi, le rotor se déplace d'un pôle par période complète et tourne à 50 révolutions par seconde.

La puissance maximum est obtenue au moteur quand son impédance est équilibrée par l'impédance du circuit de l'amplificateur. L'impédance du débit interne d'une lampe se réduit presque uniquement à une résistance; ainsi la composante réactive de l'impédance du moteur est compensée par un condensateur en série entre l'enroulement du moteur et le filament. Le courant de plaque passe par une bobine de réaction connectée à la borne commune de l'enroulement du moteur et du condensateur. Un dispositif quelque peu meilleur consisterait à mettre en résonance l'enroulement du moteur par un condensateur placé en parallèle et de brancher la batterie de plaque sur une prise de l'enroulement. De la sorte on peut se dispenser de la bobine de réaction et on peut régler le rapport du courant alternatif au courant continu de telle façon que la résultante s'annule une fois par période. En même temps, l'impédance de la lampe peut être compensée (matched) exactement, comme il le faut pour avoir au moteur le maximum de puissance.

Il serait simple de disposer un train d'engrenage de façon que ce moteur puisse actionner directement le mécanisme d'horlogerie. Toutefois, puisqu'il existait déjà des horloges destinées à fonctionner avec un courant à 100 périodes, on pensa qu'il était plus simple de les utiliser telles quelles. En conséquence, le moteur à haute fréquence commande un petit générateur, dont le rotor bipolaire est monté sur le même arbre. Puisque la vitesse du moteur est de 50 tours par seconde, et puisque le générateur est du type inducteur, la fréquence engendrée est de 100 — ou exactement 1/500 de la haute fréquence originale. Le débit de ce générateur est amplifié par une simple lampe qui fournit une puissance suffisante pour actionner le moteur de l'horloge à 100 périodes, dont la fréquence est donc une mesure exacte de la fréquence de l'oscillateur à 50,000 périodes.

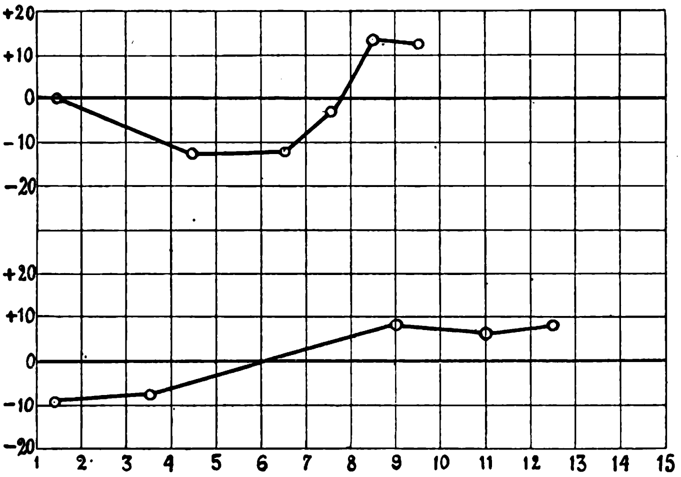

Fig. 12. — Variation quotidienne dans la fréquence de l'étalon à 50 kilocycles.

Dans la détermination des circuits électriques, on a pourvu à la production de puissance à partir d'amplificateurs multiples à 50,000, 5,000 et 100 périodes, dont les fréquences sont exactement dans un rapport harmonique. Celles-ci servent continuellement pour faire des comparaisons de fréquences dans ces trois régions.

En plus de cela, on a construit des circuits qui permettent de contrôler toute fréquence qui est un multiple ou un sous-multiple jusqu'au dixième ordre de n'importe laquelle des trois fréquences d'origine. Beaucoup de multiples fractionnaires peuvent être également réglés. Cela étend considérablement l'utilité du nouvel étalon puisqu'il est possible de fournir un courant alternatif de fréquence constante sur une étendue de 10 à 500,000 périodes par seconde. Ces fréquences ne sont pas seulement constantes; mais elles sont encore connues avec le même degré de précision que la fréquence étalon de 50,000 périodes, étant un multiple exact et rationnel ou une fraction, déterminée à l'avance, de cette fréquence.

A cause de la brièveté du temps pendant lequel cet étalon a été en service et à cause de nombreuses interruptions faites en vue d'introduire de légères modifications, on ne possède pas de données relatives à un fonctionnement prolongé. Les mesures faites sur quelques courtes périodes sont cependant intéressantes car elles indiquent ce qu'on peut attendre du système. La figure 12 donne les résultats de deux courts essais. Sur ce graphique, chaque point représente l'écart de la fréquence moyenne pour un intervalle de vingt-quatre heures, de la fréquence moyenne pour toute la période. Il est probable qu'on doive attribuer pour une large part à des variations dans la pression atmosphérique les variations dans la fréquence. Il est évidemment désirable de trouver le moyen d'employer le cristal à pression et à température constantes.

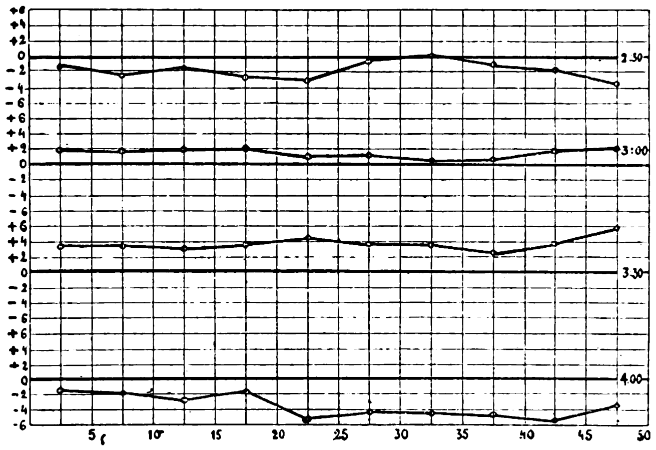

Fig. 13. — Variation relative de la fréquence d'oscillateurs semblables, contrôlés par un cristal.

Les données ci-dessus indiquent un haut degré d'invariabilité de la fréquence moyenne de cet étalon, sur de longs intervalles de temps. Comme le rapport entre la fréquence de l'étalon et le jour solaire moyen est élevé, il est désirable qu'on obtienne des vérifications supplémentaires quant à la constance de la fréquence sur de plus courts intervalles de temps. Dans ce but un second oscillateur, semblable à celui déjà décrit, fut réglé à peu près à la même fréquence, la différence étant actuellement de 0.2 période par seconde. Les nombres obtenus en mesurant la durée de dix périodes consécutives, à des intervalles d'une demi-heure, montrent les variations relatives. Les résultats de ces mesures sont représentés sur la figure 13. Chaque point représente la différence entre le rapport des fréquences pour chaque intervalle et le rapport moyen des fréquences pour l'intervalle entier de deux heures. Il faut observer que pour quelques essais, surtout ceux faits à 3 heures, le rapport des fréquences reste pratiquement le même. Cependant, à d'autres moments, comme à 2 h. 30 et 3 h. 30, il y a une périodicité marquée de la variation. Cela peut être exprimé en fonction de la variation de température; la périodicité de la variation est pratiquement la même que celle de la marche des thermostats. Quand les deux thermostats ne fonctionnent pas en phase, on peut observer une variation dans le rapport des fréquences; quand ils fonctionnent en phase, leurs fréquences restent pratiquement constantes.

Par le fait que la précision avec laquelle on peut définir une fréquence dépend de sa stabilité, de la précision de la comparaison de sa fréquence avec celle de la rotation de la terre, et de la précision avec laquelle on peut déterminer cette dernière fréquence, il semble raisonnable, en possession des données ci-dessus, d'attendre pour un proche avenir la possibilité de définir une fréquence à un instant quelconque avec une approximation absolue d'au moins un dix-millionième.

Il avait été envisagé de déterminer la fréquence en fonction de quelque autre quantité comme l'accélération de la pesanteur, la vitesse de la lumière, ou le produit de la self-induction par la capacité. Ce sont, en effet, des mesures indirectes parce que chacune des autres quantités doit d'abord être définie en fonction du temps.

Avec la précision qu'on peut obtenir actuellement dans les mesures de fréquence, il est possible à présent de renverser le processus et de mesurer certaines de ces autres grandeurs physiques en fonction de la fréquence, avec une précision qui n'avait pas encore été atteinte jusqu'à présent.

16 novembre 1927.

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Dans cette bibliographie, nous ne voulons pas mentionner toutes les contributions qui se rapportent aux mesures de fréquence. Nous avons plutôt tenté de choisir quelques travaux qui décrivent d'une manière générale les diverses phases importantes du problème. Dans la plupart des cas chacun de ceux-ci est accompagné d'une bibliographie étendue.

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Ionisation dans la haute atmosphère (1),

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Les principaux agents qui produisent vraisemblablement l'ionisation de la haute atmosphère terrestre sont : les rayons ultra-violets, les particules α et β, tous d'origine solaire, les radiations d'origine cosmique et les radiations ionisantes d'origine terrestre. Ces dernières peuvent sans doute être mises hors cause immédiatement par le fait que la conductibilité des couches atmosphériques basses augmente rapidement avec la hauteur, pour les quelques premiers kilomètres. Les effets possibles de ces agents ionisants ont fait l'objet de travaux de Chapman et Milne, de Benndorf, Elias, Lassen et d'autres. Des expériences récentes sur les ondes électromagnétiques des radiocommunications, simultanément avec des théories de la propagation de ces ondes à la surface de la terre, ont conduit à des renseignements plus précis qu'autrefois sur l'ionisation de la haute atmosphère et il a été intéressant de réexaminer les causes de l'ionisation.

Les expériences de Breit et Tuve avec des ondes de 70 mètres, d'Appleton avec des ondes de 400 mètres, de Heising avec des ondes de 57 et 111 mètres, de Wagner et Quack avec des ondes de 15 et 16 mètres, de Hollingsworth et Eckerslay avec des ondes longues, et les expériences de Taylor sur la portée indirecte des ondes de moins de 50 mètres et les considérations théoriques de Taylor et Hulburt sur ces portées indirectes, montre que la densité des électrons N augmente avec la hauter Z au-dessus de la surface de la terre, et atteint une valeur de 4.10^5 environ; au-dessus de la hauteur où N a cette valeur, la densité des électrons n'est pas connue, on sait seulement qu'elle ne continue pas à augmenter. Quoique les données qu'on peut obtenir par l'étude de la propagation des ondes ne soient guère abondantes, on peut admettre que, pendant la durée du jour (pour la zône septentrionale tempérée), la hauteur où N est de l'ordre de 10^5 est environ de 150 kilomètres et celle où N est de l'ordre de 10^3 est environ 60 kilomètres.

Par suite de la variation diurne de l'ionisation, nous considérons les rayons ultra-violets du soleil comme étant le premier agent ionisant à mettre en ligne de compte. Pour faire un calcul explicite de l'ionisation, la température, la pression, la nature des gaz constituants et leurs pressions partielles doivent être connues à chaque hauteur de la haute atmosphère. Nous pouvons admettre que ces facteurs sont complètement donnés dans la théorie classique de l'équilibre thermodynamique isotherme de Humphreys, Jeans et d'autres. Il y a doute quant à l'existence de l'hydrogène dans la haute atmosphère, mais les conclusions seront très semblables, que l'hydrogène s'y trouve ou non. Il faut connaître la loi de recombinaison des électrons avec les ions positifs. Pour cela, on dispose de la théorie de la recombinaison de J. J. Thomson et il s'y trouve des formules détaillées qui sont valables. On doit encore admettre la possibilité pour l'électron de se fixer sur une molécule neutre, car lorsqu'il agit ainsi pour produire un ion négatif, il cesse d'être un agent de réfraction énergique pour les ondes électriques. La molécule d'oxygène est la seule importante à ce point de vue, et les valeurs du coefficient d'attachement mesuré en laboratoire pour des pressions de 10 millimètres de mercure et davantage, doivent être extrapôlées aux pressions inférieures à 10^{-2} millimètres, extrapolation peut-être hasardeuse. On doit tenir compte de la diffusion des ions et des électrons qui a une très grande influence. En utilisant tous ces facteurs on conclut que la lumière ultra-violette solaire est une cause nécessaire et insuffisante de la couche de Kennelly-Heaviside, et qu'il est inutile de recourir, sauf peut-être dans des cas extraordinaires, à des hypothèses concernant d'autres agents d'ionisation, tels que des particules chargées émises par le soleil, la radiation pénétrante, etc. En supposant que l'énergie ultra-violette solaire soit celle d'un corps noir à 6,000° K et en utilisant les coefficients d'absorption, connus ou estimés, des gaz de l'atmosphère pour des ondes ultra-violettes inférieures à $\alpha = 1{,}300$ Å qui provoquent l'ionisation photoélectrique, la courbe des électrons, quand le soleil est à son apogée, c'est-à-dire à midi, en été, dans les régions tempérées, présente un maximum correspondant à 3×10^5 électrons par cm^3, à une hauteur de 190 kilomètres. Plus bas, l'ionisation ne peut pas être calculée exactement, à cause du manque de faits que pourront fournir les expériences de laboratoire ultérieures, mais il semble qu'une densité de 10^4 à 10^5 électrons par cm^3, ou 10^9 à 10^{10} ions par cm^3 (ou un mélange convenable d'ions et d'électrons) soit possible en dessous de 100 kilomètres, par exemple. On montre que cette ionisation explique quantitativement un grand nombre de faits en radiotélégraphie, notamment, les portées indirectes, les coefficients d'absorption en l'air, les ondes limites, les portées et les hauteurs apparentes atteintes par les ondes. En tenant compte des variations saisonnières dans les pressions atmosphériques des régions élevées et de l'altitude du soleil, on trouve que la couche électronique à midi, en hiver, présente un maximum de $1{,}42.10^5$ à une hauteur de 147 kilomètres, et l'ionisation en dessous de cette hauteur est inférieure d'un facteur 2 environ aux valeurs pour l'été. Après le coucher du soleil, on trouve que l'ionisation maximum diminue d'un facteur 6 environ dans les premières heures du matin; en dessous du maximum, la diminution est plus forte. On montre que ces variations saisonnières et diurnes dans l'ionisation sont en bon accord avec les variations correspondantes dans les phénomènes de propagation des ondes radiotélégraphiques.

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(1) Extrait d'un travail lu à la séance publique de l'U. R. S. I. à Washington, D. C., octobre 1927.

On trouve que l'énergie potentielle de l'ionisation diurne dans une colonne atmosphérique de 1 cm^2 est d'au moins 1 erg, et l'hypothèse, suivant laquelle moins de 1 % de celle-ci est libérée sous forme de lumière, entrerait en ligne de compte pour expliquer la luminosité du ciel pendant la nuit, ou l'aurore *non polaire* (*non polar aurora*), comme Rayleigh l'appelle. A cause de la diffusion des ions le long des lignes de force magnétique, la poussière ionique élevée de la couche ionisée diffuse vers les pôles magnétiques, se concentre là et provoque l'*aurore*. Le calcul montre que l'*ozone* de l'atmosphère n'est peut-être pas directement lié à l'ionisation précédente, mais est formé par des longueurs d'ondes plus longues de la lumière ultra-violette de $\lambda = 1{,}300$ à 1,800 Å.

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Un enregistreur automatique pour mesurer l'intensité des signaux de T. S. F. et des perturbations atmosphériques,

par E. B. JUDSON,
Laboratory for Special Radio Transmission Research,
Bureau of Standards,
Washington (D. C., U. S. A.)

Nous décrivons ici un système enregistreur établi dans le but d'observer l'intensité des signaux de T. S. F. pendant des périodes considérables, en se dispensant de la présence d'un observateur. On peut avec l'appareil utilisé, obtenir pendant vingt-quatre heures, des observations horaires de l'intensité du champ de différentes stations et des perturbations atmosphériques.

L'appareil tout entier est commandé par une horloge qui actionne, pendant chacune des diverses périodes de cinq minutes comprises dans l'heure, une série de relais; ceux-ci déclanchent automatiquement le dispositif de réception et les amplificateurs, et accordent les circuits sur les stations désirées.

La figure 1 donne le schéma du circuit et la figure 2 montre l'agencement des appareils.

Le dispositif de réception est du type appelé autodyne avec deux circuits accordés possédant une gamme d'accord qui s'étend de 60 kc. (5,000 m.) à 12.0 kc. (25,000 m.). On utilise deux antennes, l'une ayant une hauteur effective de 16 mètres, pour la réception des stations transatlantiques et des perturbations atmosphériques, l'autre ayant, par contre, une hauteur effective de $1^{m}5$ et destinée aux stations américaines voisines.

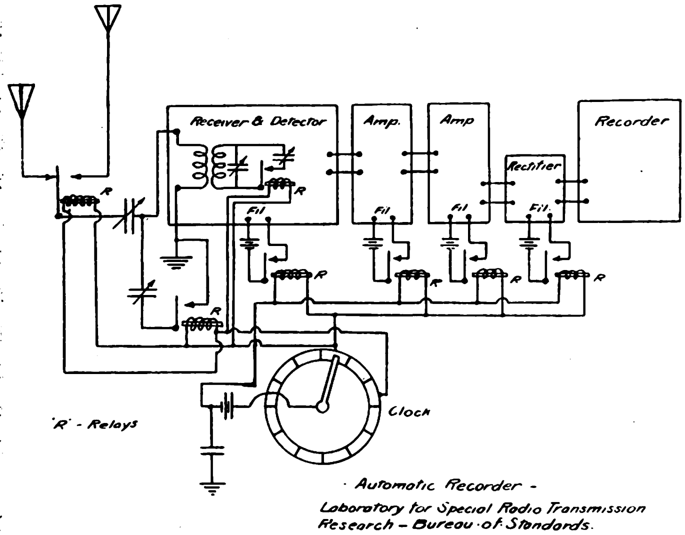

FIG. 1.

Pour assurer une grande constance de la sensibilité, on n'utilise que l'amplification basse fréquence. Celle-ci comporte deux étages de couplage par transformateur (transformer coupling) suivis de quatre étages de couplage par résistance-capacité. Grâce à cette disposition aussi longtemps que tous les courants de chauffage des filaments demeurent constants ainsi que les tensions de plaque, le système reste identique à lui-même avec assez de constance (10 %) pendant des périodes de plusieurs mois.

Le dernier amplificateur basse fréquence est couplé par un transformateur basse fréquence sans fer (an air core audio frequency transformer) à un circuit détecteur, qui renferme le galvanomètre enregistreur. Une triode, dont la plaque et la grille sont connectées, sert à la détection. En général, des lampes utilisées ainsi donnent une légère déviation du galvanomètre, due à la vitesse initiale des électrons, même quand il n'y a pas de signal. Si ce courant est de l'ordre de celui produit par les signaux, on peut l'équilibrer en plaçant une différence de potentiel inverse en série avec le galvanomètre.

Les enregistrements de la variation d'intensité des signaux et des atmosphériques se font à l'aide d'un galvanomètre à fil de Cambridge et Paul.

Cet appareil est, en principe, un galvanomètre enregistreur sensible dont la bobine mobile est disposée de façon à donner une série d'enregistrements instantanés des déviations du galvanomètre. Cette bobine est munie d'un index qui se trouve au-dessus du tambour portant le papier; entre l'index et le tambour, un fil imbibé d'eau est tendu parallèlement à l'axe du tambour, à une faible distance de la surface de ce dernier. Un petit marteau, placé au-dessus de l'index du galvanomètre, est, en temps normal, retenu à distance de cet index par une came et ses accessoires. A des intervalles réguliers, la came fait une demi-révolution permettant au marteau de s'abaisser sur la pointe, et le relève aussitôt jusqu'à sa position normale. Quand le marteau tombe, il entraîne l'index sur le tambour. Le fil encré est pincé entre l'index et le papier, ce qui produit un point sur celui-ci; ce point constitue un enregistrement visible de la déviation du galvanomètre, à l'instant donné. Le tambour de l'enregistreur est monté pour deux vitesses, l'une correspondant à une révolution complète en vingt-quatre heures, et l'autre à une révolution en deux heures, en vue de l'enregistrement pour de petits intervalles de temps. On utilise ici la révolution en vingt-quatre heures.

Le cadran de l'horloge qui commande tout le système, est constitué de douze segments de laiton disposés en cercle, chaque segment correspondant à cinq minutes. Un ressort, placé à l'extrémité de l'aiguille des minutes, forme contact électrique avec chaque segment pendant qu'il le franchit.

La came permet au marteau de s'abaisser une fois toutes les trente secondes, marquant sur le papier dix points par segment de cinq minutes.

Dans le cas actuel, le premier segment de l'heure est connecté de telle manière que, lorsque l'aiguille des minutes passe dessus, un relais se ferme, mettant en service le dispositif de réception qui est accordé sur la station dont la fréquence est la plus élevée parmi celles qui doivent être mesurées. Les amplificateurs, le tube détecteur et le marteau sont commandés simultanément, à l'aide d'autres relais.

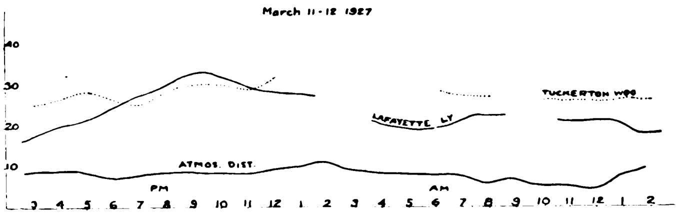

Fig. 3.

Les segments suivants actionnent les mêmes relais que le premier, mais sont connectés de manière à fermer en plus d'autres relais, qui mettent en parallèle, avec les condensateurs d'accord primaire et secondaire du dispositif de réception, des condensateurs variables mis euxmêmes en parallèle. De la sorte, le récepteur peut être accordé sur d'autres fréquences, mais ne l'est en réalité que pendant l'intervalle de temps qui correspond à un certain segment de l'horloge.

Le système est légèrement influencé par les variations de hauteur du son. La hauteur du son de battement des signaux enregistrés est, en conséquence, réglée à 1,000 périodes, par comparaison avec un diapason entretenu électriquement à la fréquence de 1,000 périodes. Une résistance d'amortissement de 30 ohms, en parallèle avec la bobine du galvanomètre, en modifie la période de façon qu'elle soit peu affectée par une variation de vitesse dans la transmission. Un étalonnage en valeur absolue du dispositif de réception s'obtient à l'aide d'un oscillateur à haute fréquence alimentant le circuit d'antenne; les amplificateurs et le détecteur peuvent être vérifiés à tout instant en les connectant à un circuit téléphonique auxiliaire (1).

La courbe d'étalonnement du galvanomètre est sensiblement une droite dans le domaine usuel des observations. Pour protéger le galvanomètre contre l'action des atmosphériques violents, on limite sa déviation à l'étendue de son échelle, en limitant le débit du dernier tube amplificateur.

Depuis février 1927, on a fait au « Laboratory for Special Radio Transmission Research du Bureau of Standards » des enregistrements continus de plusieurs stations et de perturbations atmosphériques. Pendant les mois d'hiver, quand ces perburbations étaient faibles, on effectua des enregistrements de Lafayette (LY) et Rugby (GBR) en même temps que de Tuckerton (WGG) ou Rocky Point (WSS). Pendant la saison des atmosphériques violents, on se borna aux mesures de quatre stations américaines, savoir deux stations du Nouveau Brunswick (WII) et (WRT), Rocky Point (WSS) et Marion (WSO), la réception s'effectuant sur la petite antenne. On utilise toutefois la grande antenne pour la réception des perturbations atmosphériques.

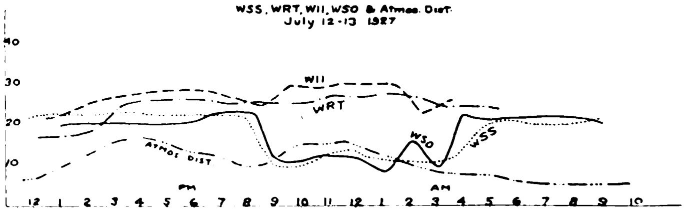

Fig. 4.

La figure 3 montre un enregistrement typique de Lafayette (LY), Tuckerton (WGG) et des atmosphériques de 12 kc. (25,000 m.).

Sur la figure 4, se trouvent représentées des courbes d'été typiques, s'étendant sur 24 heures, des stations WSS, WRT, WII, WSO et d'atmosphériques, qui montrent dans le cas de WSS et WSO, une interférence apparente des ondes réfléchies et directes pendant la nuit.

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(1) *Proc. I. R. E.*, vol. 12, n° 5, p. 521; 1924.

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Expériences relatives au calibrage des radio-compas,

Extrait d'un mémoire de F. A. KOLSTER,
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A l'heure actuelle, le navigateur reconnaît généralement la précision et l'importance du radio-compas comme instrument de navigation. Par le fait que le radio-compas, une fois installé à bord, est nécessairement placé au milieu d'un réseau métallique complexe, dans lequel naissent des courants induits par le rayonnement électro-magnétique de la station émettant les signaux, il est nécessaire de régler l'appareil de façon à pouvoir appliquer à la position observée, la correction convenable.

L'écart de la position réelle est dû à l'influence de la masse métallique du navire et l'expérience a montré que cet écart suit très étroitement une sinusoïde, ayant des valeurs positives au-dessus de l'épaule de tribord et du quartier de babord et des valeurs négatives au-dessus de l'épaule de babord et du quartier de tribord.

La précision dépend de la permanence de l'étalonnage et il importe donc que le vaste système formé des mâts, voiles d'étai et agrès, reste toujours semblable à lui-même. Il faudrait isoler les grandes spires constituant des circuits fermés, et il faudrait soigneusement relier les retenues des mâts à la coque du navire pour éviter les influences électriques variables.

Les architectes en constructions navales et les constructeurs de navires devraient avoir leur attention attirée sur ce problème, afin que l'on puisse accorder un soin suffisant au gréement des navires du point de vue des facteurs influençant le fonctionnement du radio-compas.

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Une théorie de la haute atmosphère et des météores (1),

par H. B. MARIS,
Naval Research Laboratory, Washington (D. C., U. S. A.)

La force de pesanteur agissant sur l'atmosphère terrestre oblige les gaz lourds à rester dans les basses régions et les gaz légers à s'élever vers de plus hautes altitudes par diffusion, au contraire, les vents, en l'absence de diffusion. maintiendraient, par la convection, l'uniformité de composition de l'air à toute altitude. Les idées classiques sur la variation de la pression atmosphérique avec l'altitude (par exemple Humphrey, Jeans, Chapman et Milne, etc.) ont été fondées sur l'hypothèse que la convection est négligeable, au moins dans la haute atmosphère, et que chaque gaz était, par la diffusion, en équilibre gravitationnel, avec sa pression partielle. Les recherches ont montrés, cependant, que la diffusion n'a d'importance qu'aux altitudes supérieures à 100 kilomètres seulement.

Les équations ordinaires de la diffusion montrent que si l'air était uniformément mélangé à toutes les altitudes et laissé ensuite indépendant de tous les courants de convection, il y aurait un perpétuel flux de molécules légères vers le haut et de molécules lourdes vers le bas, qui serait indépendant de l'altitude, jusqu'à ce qu'un niveau fût atteint où les gaz diffusés seraient en équilibre gravitationnel. Ce niveau de « diffusion » pour l'hydrogène descendrait de l'infini jusqu'à 142 kilomètres en un jour : après cinq jours, il serait à une hauteur de 127 kilomètres et après 50 jours il serait à 113 kilomètres. Les niveaux correspondants pour l'hélium seraient respectivement de 137, 120 et 106 kilomètres. Les calculs nouveaux donnent pour les constitutions en hydrogène et hélium, au-dessus de 150 kilomètres environ, 1/100,000 des valeurs calculées antérieurement.

L'emploi récent de la haute atmosphère comme milieu de transmission des radiations électromagnétiques de longueurs d'onde de 10 à 10,000 mètres, a accru l'importance de la connaissance ou du moins d'une théorie des variations qui se produisent dans cette haute atmosphère entre les conditions diurnes et nocturnes, pendant différentes saisons de l'année. L' absorption du rayonnement solaire et terrestre par l'air doit dominer toute théorie de la distribution de la température dans la haute atmosphère. Humphrey a discuté ce problème et a suggéré qu'il aurait été résolu, mais aucune tentative ne fut faite préalablement pour appliquer les coefficients de rayonnement et d'absorption, et tirer au clair le comportement thermique de la haute atmosphère, ni pour évaluer la température probable à des altitudes supérieures à 20 kilomètres pour les conditions de rayonnement de jour et de nuit, ou d'hiver et d'été.

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(1) Extrait d'un travail lu à la séance publique de l'U. R. S. I. à Washington, D. C., octobre 1927.

La vapeur d'eau au-dessus de 11 kilomètres absorbe un peu plus de 20 % du rayonnement du corps noir venant d'en-dessous, aux températures terrestres, tandis que l'anhydride carbonique en absorbe presque 40 %. L'ozone absorbe environ 2 % mais sa présence est importante parce qu'il absorbe environ 4 % du rayonnement solaire à une altitude où la plus grande partie de la radiation doit être reproduite par l'ozone lui-même. Les calculs de température basés sur ces coefficients d'absorption montrent qu'à une latitude de 50°, au-dessus d'une hauteur de 60 kilomètres, nous devrions nous attendre à une température d'environ 250° K pendant un jour d'hiver, avec un abaissement allant jusqu'à 220° pendant la nuit, et une température de 370° pendant un d'été avec un abaissement correspondant jusqu'à 230° pendant la nuit. L'atmosphère à la base de la stratosphère ne peut pas être en équilibre radiatif, mais doit recevoir plus d'énergie rayonnante qu'elle n'en perd, tant d'en haut que d'en bas pendant une journée de vingt-quatre heures. La perte de chaleur, par rayonnement, de l'équateur chaud, est bien moindre que des régions polaires froides. Une augmentation de la température au niveau de la mer, près de l'équateur, ne produirait pas une augmentation de l'énergie perdue par le rayonnement de ces régions, mais produirait effectivement une diminution. La perte de chaleur par rayonnement de la terre dépend, non pas des conditions à la surface, mais de la température à la base de la stratosphère et de l'absorption dans la stratosphère. Le comportement de la température de la surface terrestre est en équilibre très instable. Une légère variation dans l'anhydride carbonique de l'air aurait une influence formidable sur la climatologie terrestre. Si la concentration de l'air en anhydride carbonique variait de la valeur actuelle de 0.03 % à celle de 0.1 % les plantes tropicales croîtraient probablement dans les régions polaires. D'autre part, si cette couche protectrice diminuait de 0.03 % à 0.01 % on trouverait probablement de la glace à l'équateur.

Puisque la théorie actuelle conduit à de faibles densités de l'atmosphère au-dessus des altitudes de 100 kilomètres, des densités cent mille fois plus petites que celles des tables classiques à 300 kilomètres, les faits relatifs aux apparences des météores exigent une explication. Il semble possible de donner cette explication en suivant en partie les idées de Sparrow et en partie celles de Lindemann. Quand un météore à grande vitesse rencontre une molécule d'air, on suppose que l'énergie du choc fait jaillir violemment des atomes, des molécules et peut-être des petites particules de dimension moléculaire, du corps même du météore. Cette matière émise, en vertu de sa vitesse, transporte dans l'air l'énergie qui produit éventuellement la lumière de la traînée du météore. Par exemple, quand une molécule d'azote frappe un météore de fer a une vitesse de 40 kilomètres par seconde, l'énergie du choc est suffisante pour élever de 1,000° C la température de 1,800 molécules ou pour évaporer 56 molécules de fer, ou encore pour évaporer et ioniser 24 molécules de fer. Il résulte de ce choc qu'une masse égale à plusieurs fois celle de la molécule d'azote est émise par le météore principalement sous la forme d'atomes de fer possédant une énergie considérable et animés de vitesses légèrement supérieures à celles du météore lui-même. Les collisions de ces atomes de fer avec les molécules de l'air donnent naissance à la traînée visible. L'énergie d'excitation de ces collisions peut être de 155 volts pour l'azote ou 280 volts pour l'argon. Une grande partie de cette énergie peut être rayonnée dans la région ultra-violette ou même dans celle des rayons X doux, et il est probable qu'il ne se trouve pas plus de un dixième du rayonnement total dans la partie visible du spectre. Donc, la masse totale du météore doit être bien supérieure à celle déduite par Lindemann et Dobson de leurs considérations sur la relation entre la masse d'un météore et sa lumière. Les variations de température dans la haute atmosphère, du soir au matin, et de l'hiver à l'été, données par la présente théorie, nous conduisent à prévoir l'apparition des météores à des altitudes qui

sont plus grandes, disons de 5 kilomètres, le soir que le matin et l'été que l'hiver. Il serait intéressant de savoir si cette différence a été observée.

Des études récentes sur la propagation des ondes électromagnétiques à la surface de la terre ont accentué la nécessité de trouver une théorie et des conclusions définies concernant les variations diurnes et saisonnières de la température, et la composition de l'atmosphère à des altitudes supérieures à 50 kilomètres. Le but de cette discussion était de faire ce qui était possible pour progresser dans ce sens.

Naval Research Laboratory,
« Bellevue », Washington, D. C.
1er novembre 1927.

* * *

Note on waves of a few meters,

par René MESNY.

During the past few years, we have had the opportunity of making a series of experiments, with the collaboration of Messrs. Beauvais and David, on waves the lengths of which lay between two and four meters. We summarize below the experiments which permitted us to find the properties of such waves and their conditions of use.

Transmission and reception. — The generator was always of the symmetrical type (1) with two triodes : the input power varied between 2 and 200 watts.

The antenna was most often of the half-wave type, but we made use also of antennas of several half-wave-lengths, and even antennas of random types, such as those of the ships which we were working.

The reception was always by super-regenerative circuits, the use of which proved itself advantageous and practical.

Propagation far from the ground. — Between two points, each of which is visible from the other, transmission is excellent at any distance, even with the lowest powers. In the Alps between two peaks 100 kilometers apart, we maintained absolutely sure telephonic duplex communication with portable battery transmitters consuming 3 watts input. In other circumstances, we reached 280 kilometers, the received signals remaining still very loud though transmitter and receiver were no longer in complete visibility.

Moreover, there is nothing surprising in these results; they are simply due to the fact that with these very short waves the energy is perfectly utilised for radiation. If W is the radiated energy in watts and D the distance in kilometers, the field at this distance is, in micro-volts per meter :

$$\mathcal{E}^{\frac{\mu V}{m}} = 6700 \frac{\sqrt{W^{w}}}{D^{km}},$$

Assuming that the transmitter is isolated in space and that there is no damping, a single watt radiated thus gives a field of 67 $\frac{\mu V}{m}$ at a distance of 100 kilometers.

Effect of screens. — When there is a screen between transmitter and receiver, the signals are the more attenuated the greater the height of the screen, and they soon vanish as it increases. We made many experiments in mountainous country by interposing between the transmitter and ourselves the screen formed by a chain of hills situated at distances of 20 to 30 kilometers. By moving on a slope opposite the transmitter we could either place ourselves within visibility of the latter, or penetrate more or less deeply into its geometrical shadow. In all cases, the signals vanished somewhat abruptly at the instant where the line joining the receiver and the edge of the screen made an angle of 1° to 1.5° with the line passing directly to the transmitter. No quantitative measurements have yet been made, and the speed of disappearance of the strong signals depends in a large degree upon the receivers used : we have the impression, however, that the decrease of the signals was more abrupt than would result from the optical formulas for diffraction by a straight screen.

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(1) R. Mesny, *Les Ondes électriques courtes*, p. 61.

Propagation near the ground. — Near flat ground the phenomena may completely change in aspect.

At first, if transmitter and receiver are both some meters distant from a plane, or nearly plane, ground, on an open space, communication with a power of 3 watts, which was very good at 100 kilometers between two peaks becomes bad at 1.5 kilometer; the signals disappear completely between 2 and 3 kilometers.

But it is not necessary that the transmitter should be near the plane, it is sufficient that the receiver should be so. We carried out numerous experiments in which the transmitter was at the top of the Eiffel Tower while we moved the receiver about the Parisian district. Every time we were on a hill-top, the signals were extremely strong at all distances. On the contrary, as soon as the receiver was on a plateau the signals disappeared when the distance was more than about 20 kilometers. If we went to that edge the plateau which was situated towards the transmitter, the signals again became strong; at a distance of 4 to 500 meters from this edge, often even at 200 meters, they disappeared completely or nearly completely.

The first assumption that comes to mind consists in seeing in this phenomenon an effect of ground absorption. The abruptness of the decrease on leaving the edge of the plateau caused us to seek another explanation. It appeared improbable that the energy of the wave could be so strongly absorbed in a travel of 400 or 500 meters. It seems to us that Zenneck's formulas, which are often invoked in such circumstances, are not applicable : they deal with a wave which has reached a steady state after an obligatory long travel near the ground, long enough for the electric field to have acquired an important horizontal component so that the wave leans forward (this picture is, moreover, lacking in precision because of the rotation of the field). Although we may try to justify such an explanation in the case where transmitter and receiver are near the ground, matters are different when the transmitter is very high : since it is at the same time more or less distant, we can assume that the waves reaching the receiver are plane waves, and when the receiver is situated upon a plateau, we are in presence of the reflection of a plane wave at a plane. This is a classical problem and it is easy to find that in the conditions of practice (dielectric constant and conductivity normal) the reflexion is made with a phase displacement of nearly 180°, if the angle of the rays with the ground is small. When this angle is e. g., half a degree, the resulting field at a distance of some hundreds meters from the edge of a plateau is nearly 25 times weaker than the field at the edge where reflexion does not operate. We think this is the cause of the observed phenomenon.

We are confirmed in this opinion by the fact that, after having moved the transmitter and placed it on a hill top of which the height was 175 meters (Mont Valérien), we could still obtain very good signals at a distance of several kilometers from the edge of a plateau of which the height was nearly 200 meters (south of Beauvais) and which was situated fifty kilometers from the transmitter. It was no longer a case of a reflected plane wave, but of a diffraction phenomenon. Moreover, the energies concerned were of the same order as in the case where the transmission took place from the top of the Eiffel Tower, with about 150 watts input.

It may be that these phenomena have no practical importance; they seem, however, interesting as demonstrating different modes of propagation.

Propagation to great distances. — Trials, executed with care in very favourable conditions, with a transmission power of 150 watts feeding did not give us any result at distances of 600 and 900 kilometers. Though the transmitter was still at the top of the Eiffel Tower, we were very far beyond the theoretical limit of visibility : the visible horizon of the Eiffel Tower is but 65 kilometers and, at a distance of 600 kilometers, the sphericity of the earth constitutes a screen of 7 kilometers height. Only ionic refraction could permit such communications and it is known that this seems

insufficient to bring back towards the earth waves with lengths inferior to some ten meters, even at very much larger distances.

Use of waves of a few meters. — The practical use of waves of a few meters is as simple as that of longer waves. The small powers which suffice for covering the accessible distances, are very easily attained; the antennas may be of any kind, but by using half-wave antennas it is possible to reduce the bulk of the gear in a way very advantageous for practical purposes. The use of super-regeneration for reception overcomes all the difficulties which might be produced by the increase of frequency. Atmospherics are absolutely imperceptible.

The radical influence of screens gives to these waves a quality of discretion which does not exist for any other wave band, and this quality can be notably increased by using directional systems, the establishment of which is especially easy.

Finally, thanks to the reduction in scale, these waves enable us to make with ease numerous experiments which are difficult or impossible with the longer waves. We may cite, in this connection, the measurements we made on different directional systems, in particular on *curtains* of antennas in fret-works (*en grecque*). This arrangement, performed by Chirein (1) and ourselves (2), enables a considerable concentration of energy to be obtained by the simplest means.

* * *

Note sur la transmission à longue distance des ondes courtes,

par T. MINOHARA et K. TANI,
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Tokyo (Japan).

On sait parfaitement que la trajectoire des ondes courtes sur de longues distances, dépend de la position de la couche dite de Kennelly-Heaviside, qui varie avec l'heure du jour et la saison. Il n'est donc pas aisé d'aborder l'étude du phénomène de transmission à longue distance, puisque la hauteur de la couche de Kennelly-Heaviside n'est pas la même en différents endroits où l'heure et la saison sont totalement différentes.

Pour étudier les résultats expérimentaux concernant les ondes courtes, dans les communications à longue distance, nous avons utilisé la méthode suivante qui convient parfaitement pour déterminer la trajectoire de l'onde.

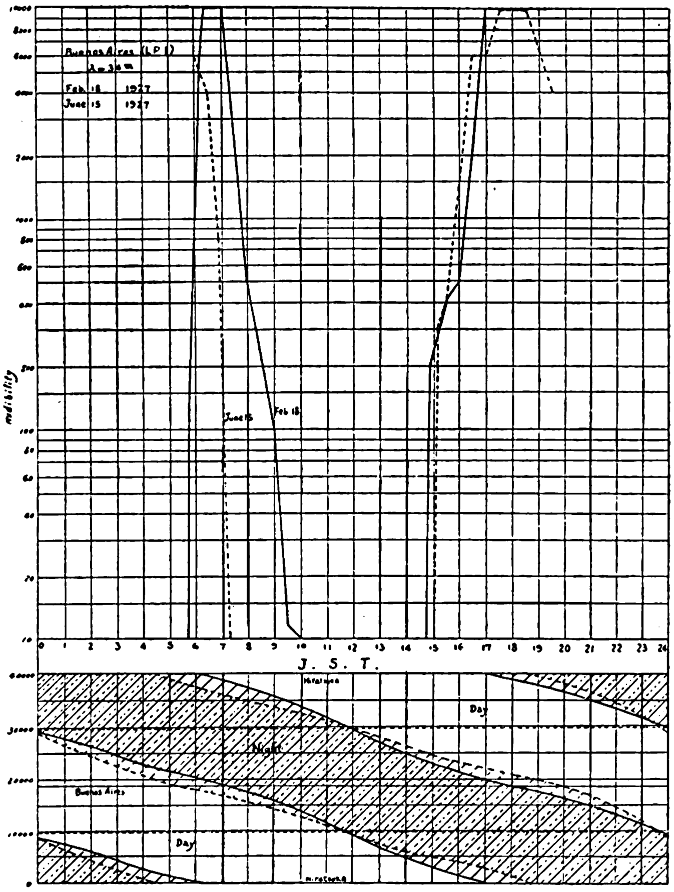

Fig. 1.

Portons en absisses les heures et en ordonnées les distances le long de l'arc de grand cercle qui relie les deux stations, comme le montre la partie inférieure des figures 1 et 2.

Dessinons la ligne de séparation entre la région de jour et celle de nuit. Nous pouvons alors rechercher aisément la relation qui existe entre la trajectoire des ondes et l'espace obscur.

De plus, nous pouvons admettre que la conductibilité de la haute atmosphère, à midi, n'est pas la même qu'à l'instant qui précède le coucher du soleil ni qu'à celui qui suit son lever. Si nous supposons que la hauteur de la couche conductrice de la haute atmosphère est inversement proportionnelle au cosinus de la latitude astronomique du soleil à l'endroit considéré, nous pouvons calculer et tracer la ligne qui représente la courbure de la couche de Kennelly-Heaviside, comme le montre la figure 3.

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(1) Société Française Radioélectrique. —Brevet français, n° 216,757, 10 mars 1926.

(2) R. Mesny, Emissions dirigées par rideaux d'antennes en grecque. (*Onde électrique*, 6 mai 1927, pp. 181-200.)

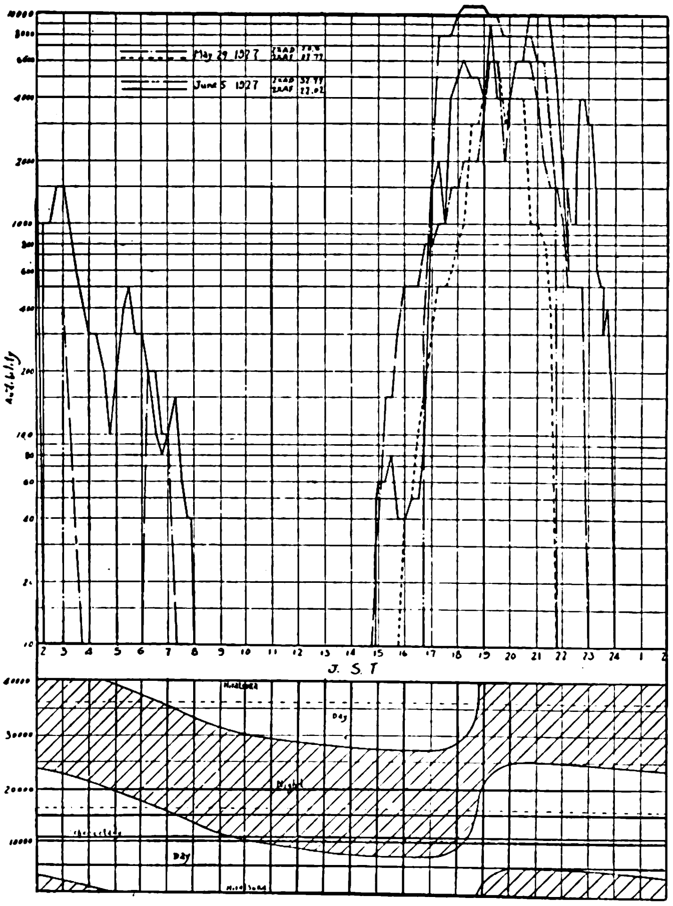

Fig. 2.

Expliquons maintenant quelques résultats expérimentaux caractéristiques à l'aide des considérations précédentes.

La figure 1 représente le résultat de la mesure des intensités de signaux de petite longueur d'onde (λ = 34 m.) envoyés par la station de Buenos-Aires (L P I), et reçus en notre laboratoire à Hiratsuka près de Tokio, le 18 février et le 18 juin 1927.

Il résulte clairement de la figure que les signaux sont intences deux fois par jour au moment où les signaux nocturnes parcourent dans les deux sens opposés la plus courte distance dans l'obscurité.

La figure 2 représente le résultat de la mesure d'intensité du signal de courte longueur d'onde envoyé par la station XAF Schenectady, et reçu à Hiratsuka.

Il faut observer que les signaux de longueur d'onde de $26^{m}8$ sont intenses à 3 heures et à 6 h. 30 du matin J. M. T. (temps moyen du Japon). Sur la figure 3, ABA' représente la transformée au développement du grand cercle passant par la station réceptrice A et la station émettrice B. La ligne *ll'*, dont la hauteur au-dessus du sol est *h*, représente le niveau de la couche de Kennelly-Heaviside pendant la nuit, et la ligne DD', dont la hauteur est *h'*, représente le niveau de la couche à midi alors que l'ionisation est la plus forte. Ainsi le niveau de la couche conductrice à un instant quelconque, quand le rayon incident fait un certain angle avec elle, doit se trouver entre *ll'* et DD'. En portant la position de la couche, calculée dans l'hypothèse énoncée ci-dessus, nous obtenons respectivement les courbes de 3 heures, 4 h. 45, 6 h. 30, 9 h. 30, 14 heures, 19 heures, J. M. T. (temps moyen du Japon) comme cela est représenté en traits interrompus sur la figure 3.

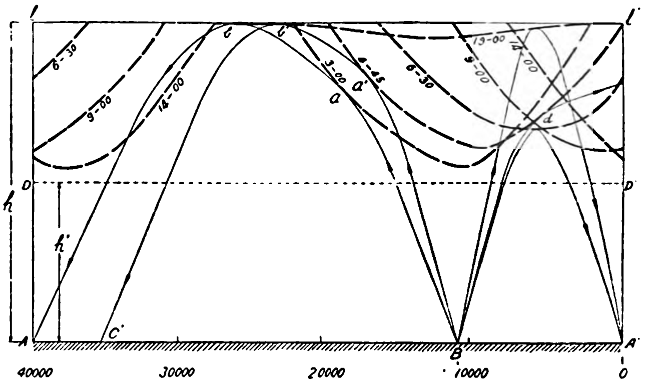

Fig. 3.

Le problème de la trajectoire des ondes ne peut être résolu ni par le calcul de la réflexion seule ni par celui de la réfraction seule, mais on peut supposer en général que le rayon est réfracté graduellement le long de la surface du globe et réfracté brusquement — ou pratiquement réfléchi vers le bas — par la couche conductrice, d'où il retourne alors vers la terre. A titre de simplification, nous réduisons, dans la représentation graphique, le processus à une pure réfraction équivalente, en supposant que la surface de la terre soit plane. Nous obtenons alors la trajectoire B*ab*A pour 3 heures J. M. T. (temps moyen du Japon) ; et B*d*A' pour 6 h. 30 J. M. T. (temps moyen du Jajon) ; le signal peut donc être reçu par le poste A ; mais, au contraire à 4 h. 45 J. M. T. (temps moyen du Japon), par exemple, nous ne pouvons pas recevoir le signal, l'onde suivant le chemin B*a'b'c'* ou B*d*.

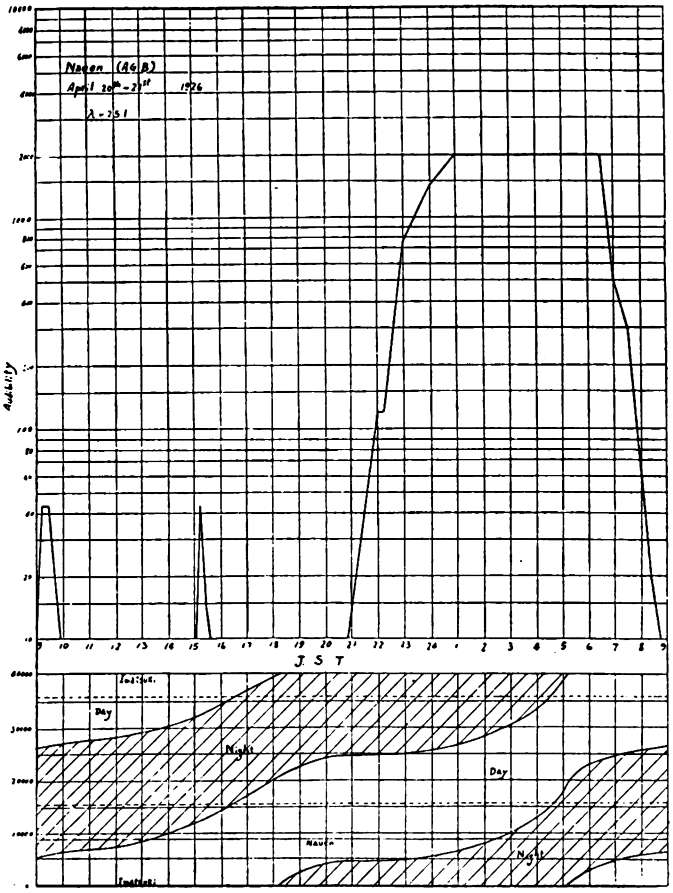

Fig. 4.

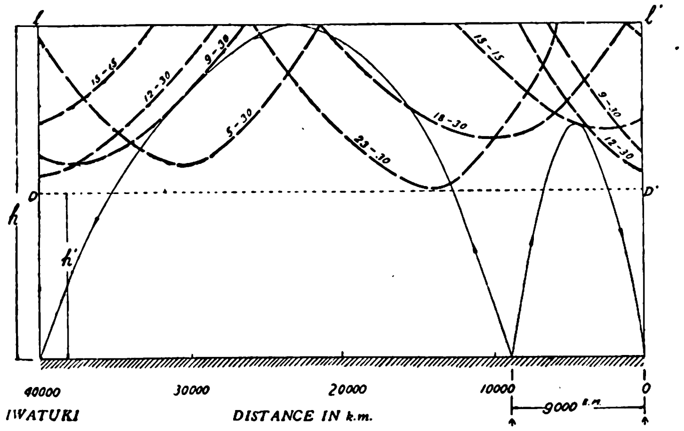

Fig. 5.

La figure 4 représente l'intensité du signal envoyé par le poste de Nauen, reçu à Iwatsuki. On peut trouver dans ces résultats un fait remarquable analogue et la même explication que celle exposée ci-dessus peut s'appliquer, comme le montre la figure 5.

Comme il y a beaucoup d'hypothèses et que des facteurs inconnus sont impliqués dans ces considérations, il n'en va probablement pas aussi simplement dans le cas actuel, mais on peut envisager ces considérations comme des suggestions nouvelles qui tiennent compte de l'état actuel des connaissances en radiotechnique.

* * *

Variation diurne dans les intensités de signaux par ondes courtes,

par T. NAKAGAMI et T. ONO,

Direction General of Telegraph and Telephone Engineering,
Ministry of Communications, Tokyo (Japan).

On a proposé beaucoup de théories sur les phénomènes de transmission des ondes courtes et on a donné les résultats des mesures faites sur les signaux par ondes courtes, résultats qui apportent une contribution appréciable à notre connaissance de la propagation des ondes électromagnétiques.

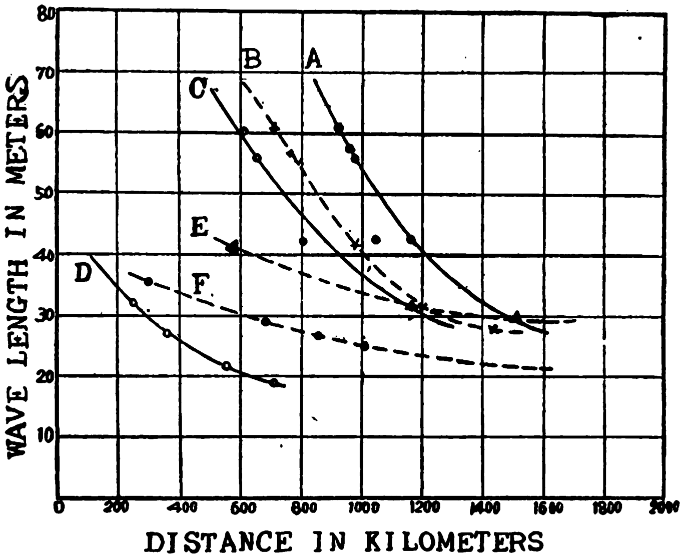

Fig. 2.

En examinant ces données, nous devons tenir compte de la nature des ondes utilisées, de la saison et de l'heure auxquelles les expériences furent effectuées, de la distance entre le poste émetteur et le poste récepteur, et comme ces facteurs sont en relation intime, les liaisons mutuelles sont également complexes.

Pendant ces deux dernière années, nous avons entrepris plusieurs essais dans le but de déterminer la meilleure longueur d'onde pour les émissions nocturnes ou diurnes, pendant une certaine saison et pour une certaine distance, au Japon.

L'un de nos essais fut effectué au début de janvier 1927. Un émetteur d'ondes courtes fut installé à la Kagoshima Radio Station; l'antenne était simplement en forme de V, et sa puisance était d'enviton 500 watts, la longueur d'onde propre étant égale à celle de l'émission. Les stations réceptrices furent choisies à divers endroits, de façon à nous permettre de recueillir des résultats à différentes distances. A chacun de ces endroits on installa un récepteur pour ondes courtes, composé d'un détecteur et d'un étage d'amplification à basse fréquence.

Nous envoyions des signaux d'essai pendant ving-quatre heures sur plusieurs longueurs d'ondes différentes, de Kagoshima, et ils étaient reçus simultanément par huit stations réceptrices. Nous avons obtenu ainsi les résultas visibles sur la figure 1 qui représente la variation diurne dans l'intensité des signaux en fonction des longueurs d'onde et des distances. Nous pouvons déduire de ces courbes les faits intéressants qui suivent :

1. Pendant la nuit, l'onde de 61 mètres est indéchiffrable à de courtes distances, mais quand la distance augmente, les signaux deviennent plus intenses et les temps de lisibilité s'allongent.

2. La portée indirecte d'une certaine onde est plus grande la nuit que le jour et les portées indirectes des ondes plus courtes sont plus grandes que celles des ondes longues.

3. Quand la longueur d'onde diminue, les temps (hours) de lisibilité la nuit diminuent de plus en plus à petite distance mais augmentent avec la distance, la longueur d'onde restant la même.

4. Il semble y avoir des longueur d'onde optima de nuit et de jour à une distance déterminée et plus la distance augmente, plus l'optimum des longueurs d'onde diminue. Nous reviendrons sur ce point.

5. Une particularité dans la transmission des ondes courtes le long du méridien pour une certaine distance et sur une certaine longueur d'onde, réside dans le fait que les signaux sont indéchiffrables sauf pendant quelques heures au lever et au coucher du soleil. La raison de ce fait paraît être que pendant le jour les signaux sont absorbés et deviennent indéchiffrables à cause de la faible puissance et que pendant la nuit les signaux dépassent ces distances, et qu'au lever et au coucher du soleil l'altitude de la couche ionisée ou le degré d'ionisation dans la haute atmosphère sont propices à la transmission à cette distance et sur cette longueur d'onde.

Nous avons encore fait plusieurs essais sur les ondes courtes pour trouver la longueur d'onde optimum pour une distance donnée. L'un des essais fut effectué en juin 1926.

Les signaux d'essais envoyés de Tokyo sur différentes longueurs d'onde furent reçus simultanément à plusieurs endroits, et nous avons déterminé la longueur d'onde qui peut être reçue pendant vingt-quatre heures en un certain endroit, quoique cette onde ne fût la meilleure ni pour la nuit ni pour le jour.

Le résultat est donné dans la table ci-dessous et dans la figure 2, courbe F.

Station émettrice	Tokyo.				
Station réceptrices	Kanagawa.	Hiroshima.	Sapporo.	Otchisi.	Kagoshima.
Distance en Km entre les stations émettrice et réceptrice	300	360	890	965	965
Longueur d'onde en mètres qui donnent les signaux toute la journée. .	35	29	28	25	25

Un autre essai fut effectué en janvier 1927 et nous avons obtenu le résultat suivant :

Station émettrice . .	Kagoshima.	Sapporo.	Kemikawa.	Kagoshima.	Kagoshima.	Osaka.	Kemikawa.
Longueur d'onde . .	61	75	56	4.28	41.5	32	27.5
Lieu de la réception où les signaux étaient lisibles pendant 24 heures.	Sud de Naha. Sud de Kanazawa.	Hiraiso. Iwatsuki.	Hiroshima. Sapporo.	Iwatsuki.	Nord de Kanazawa.	Naha.	Sud de Naha.
Distance entre la station émettrice et le lieu de la réception.	700	780	800	985	900	1,170	1,550

La courbe B de la figure 2 est tracée d'après la table ci-dessus. A côté de cette courbe, les autres de la figure 2 représentent les résultats obtenus par des essais analogues entrepris au Japon; ainsi, la courbe A représente la gamme des maxima diurnes de l'émetteur de 500 watts en hiver; la courbe D, la portée indirecte pendant le jour; la courbe C, celle pendant la nuit en hiver; et la courbe E, les longueurs d'onde qui peuvent être utilisées pendant le jour et la nuit au printemps. Si nous voulons choisir la longueur d'onde optimum pour des services commerciaux, nous devons considérer les courbes B, E et F en hiver, au printemps et en été, respectivement, en tenant compte du fait que la longueur d'onde optimum la nuit est plus longue de quelques mètres que celle donnée par la courbe, alors que la meilleure longueur d'onde le jour est plus courte de quelques mètres.

T. NAKAGAMI et T. ONO.

FIG. 1 - A

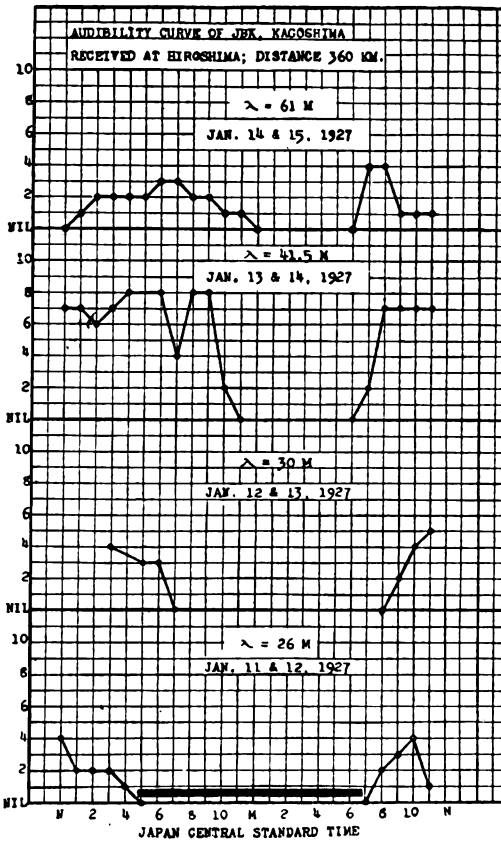

FIG 1 - B

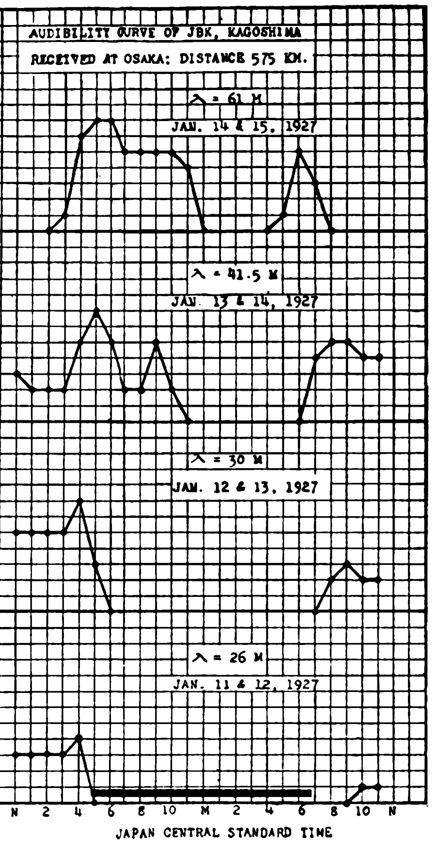

FIG. 1 - C

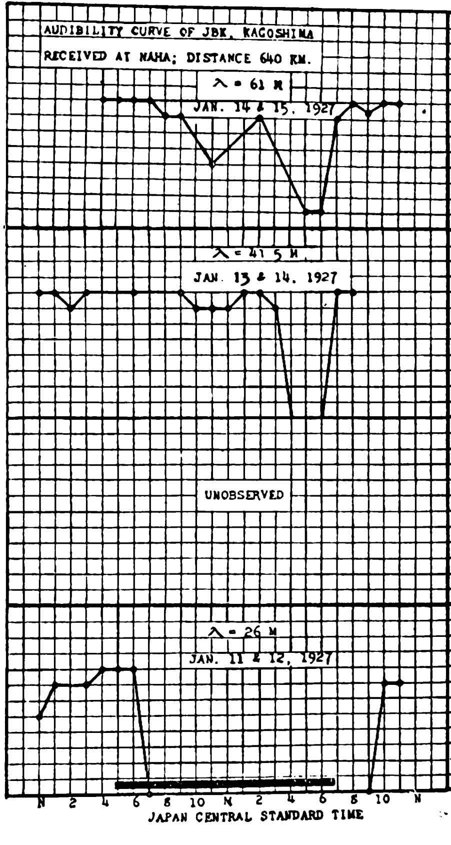

FIG. 1 - D

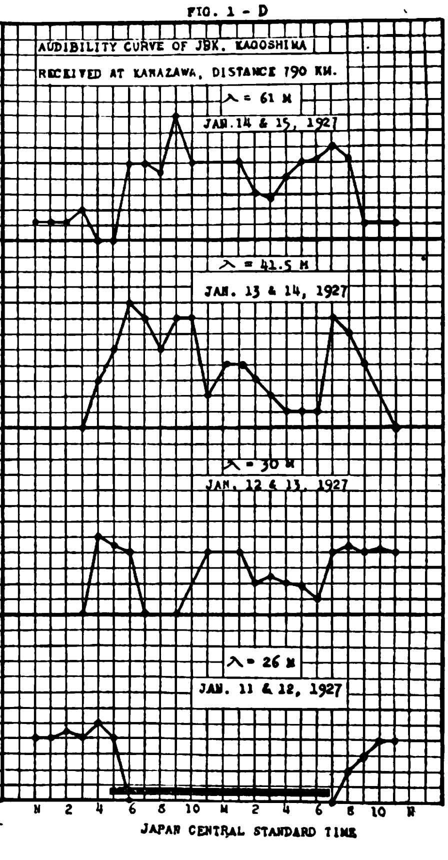

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FIG. 1 - E

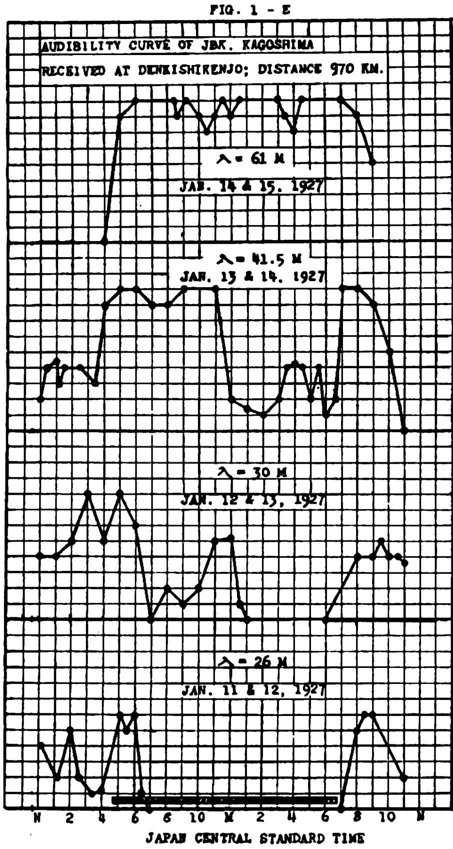


FIG. 1 - F

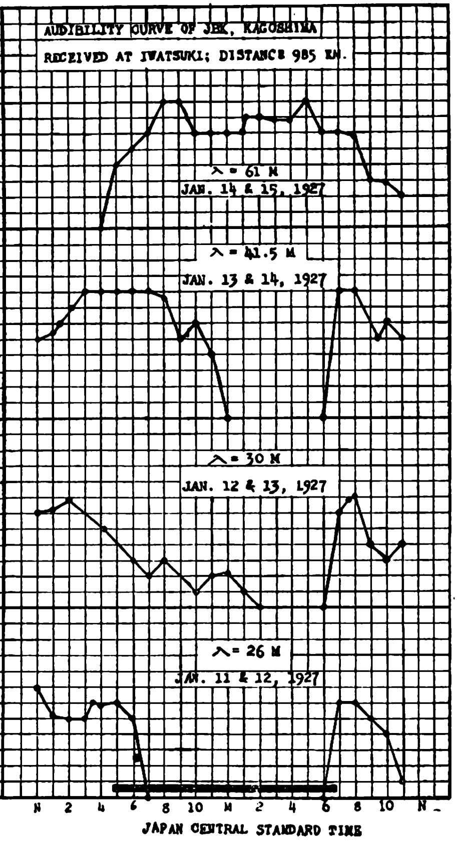


FIG. 1 - G

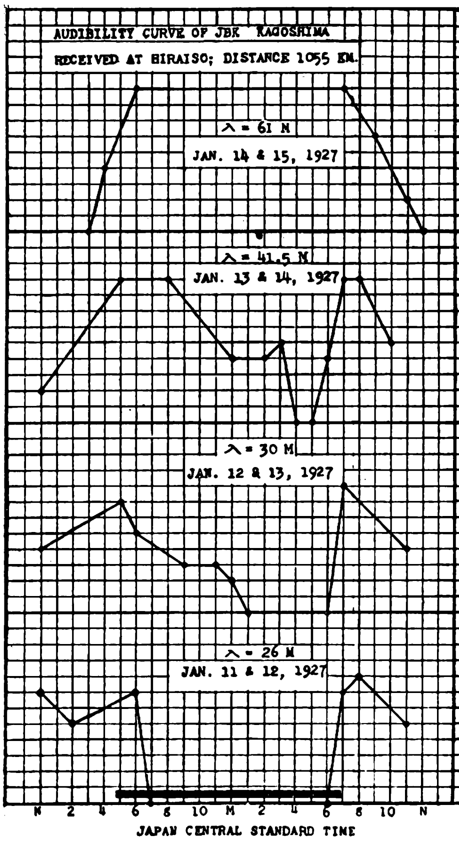
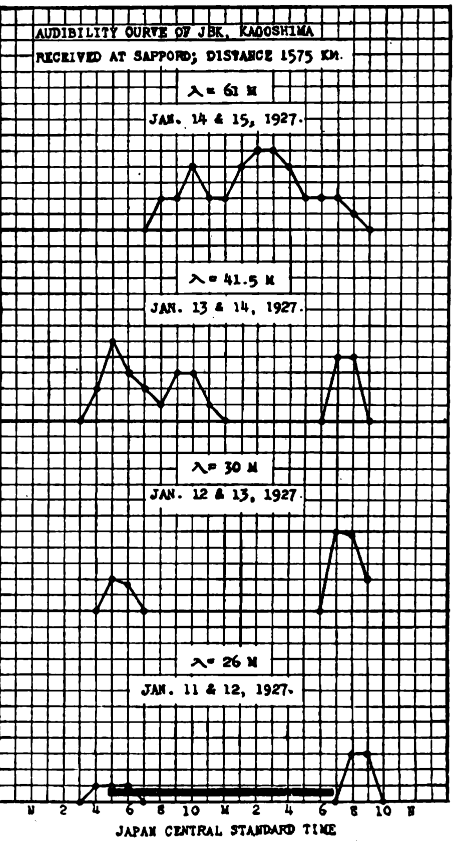


FIG. 1 — H

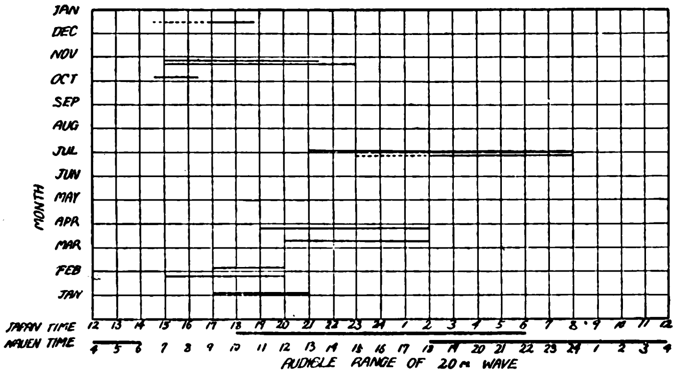


Variation saisonnière dans l'intensité des signaux de longueur d'onde de 20 mètres du poste de Nauen, au Japon,

par T. NAKAGAMI et T. ONO,
Direction General of Telegraph and Telephone Engineering, Ministry of Communications, Tokyo (Japan).

Nous avons fait une série d'essais de réception des ondes de 20 mètres du poste de Nauen, à la station réceptrice Iwatsuki près de Tokyo, pendant plus d'un an, et nous avons obtenu le résultat qui est résumé dans la figure ci-contre. Les heures pendant lesquelles on pouvait lire les signaux de Nauen, sont représentées pour plusieur saisons; nous tentions de recevoir les ondes de 20 mètres de Nauen pendant vingt-quatre heures.

En été, le signal est lisible pendant près de onze heures, quand la nuit recouvre la trajectoire qu'on suppose située sur l'arc de grand cercle allant de Nauen au Japon; mais en hiver, cette durée est réduite à quelques heures, variant avec le temps, quand il fait jour le long de l'arc de grand cercle qui passe par Nauen et le Japon. Le grand cercle qui passe par le Japon et l'Allemagne est presque tangent au cercle arctique en son milieu, de telle sorte que la nuit, en été, n'est pas complètement obscure et que le jour, en hiver, n'est pas complètement clair, ressemblant tous deux au crépuscule. Cela étant, le degré d'ionisation de la haute atmosphère est presque le même pendant le jour d'hiver et la nuit d'été; donc les conditions de propagation des signaux semblent déterminées presque par les mêmes facteurs.



* * *

Relation entre la réception en T. S. F., la position et l'aire des taches solaires,

par GREENLEAF W. PICKARD,
Wireless Specialty Apparatus C°, Boston (Mass., U. S. A.)

Depuis quelques années à peine, quelques mois même, la relation entre la réception radiotélégraphique et l'activité solaire est définitivement connue (1). Lorsque l'activité solaire augmente, ce qui nous est indiqué par l'augmentation du nombre de taches solaires, par la constante solaire ou le rayonnement ultra-violet, il se produit une élévation d'intensité de la réception diurne, et une diminution d'intensité de la réception nocturne. Si l'on prend des moyennes quotidiennes périodiques, on peut établir des relations générales qui subsistent d'un jour à l'autre. Par exemple, le passage au méridien central d'un groupe de taches solaires s'accompagne généralement d'un affaiblissement de la réception nocturne et (exception faite de ce qui s'est passé en 1926 et dans la première partie de 1927) d'une légère augmentation du champ diurne (2).

Le rapport entre les évaluations de l'activité solaire et celle de l'intensité de la réception nocturne est particulièrement frappant dans la gamme de la radio-diffusion comprise entre 750 et 1,500 kilocycles, pour les voies d'émission Est-Ouest. Mais même avec ce type d'émissions spécialement sensibles, le coefficient de corrélation diminue rapidement quand on prend des intervalles de moins de un mois. Si on compare aux nombres de Wolfer (Wolfer Provisional Sunspot Numbers) l'intensité de la réception nocturne à Newton Center (Massachusetts), des stations WBBM et WGN de Chicago, pendant la période qui va de janvier 1926 à septembre 1927, on trouve les corrélations suivantes :

INTERVALLE	COEFFICIENT DE CORRÉLATION	r/e
Trois mois . . .	— 0,92 ± 0,038	24,0
Deux mois . . .	— 0,75 ± 0,093	8,1
Un mois . . .	— 0,54 ± 0,10	5,4
Quinze jours . .	— 0,45 ± 0,084	5,4
Sept jours	— 0,030 ± 0,06	5

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(1) The Correlation of Radio Reception with Solar Activity and Terrestrial Magnetism. (*Proceedings of the Institute of Radio Engineers,* vol. 15, n° 2; February 1927.)

(2) The Correlation of Radio Reception with Solar Activity and Terrestrial Magnetism. (*II. Bulletin National Research Council,* n° 61, July 1927, *Proceedings of the Institute of Radio Engineers,* vol. 15, n° 9, September 1927.)

Quand on compare d'un jour à l'autre l'intensité de la réception et les nombres de Wolfer, on trouve un faible coefficient de corrélation. Si l'on tient compte de la distance β entre les taches et le centre du soleil, et si l'on multiplie l'aire de ces taches par (1 — β), on obtient un coefficient de corrélation un peu plus grand. On augmente encore ce coefficient en se bornant à considérer les taches qui sont comprises dans un rayon de quelques degrés du méridien central. Cependant, quand bien même on considère à la fois la position et l'aire des taches, la relation entre le passage de groupes individuels de taches solaires et l'intensité de la réception n'est pas assez précise pour traduire un rapport de cause à effet.

A titre d'hypothèse de travail, j'ai supposé que certaines zônes solaires déterminées provoquent les perturbations dans la réception. Ces zones subsisteraient pendant un certain nombre de rotations solaires. En général, ces zônes ou aires actives sont aussi les zones de formation des taches solaires; mais elles engendrent des perburbations dans la réception même quand elles ne renferment pas de taches. D'autre part, d'importants groupes de taches peuvent naître en dehors de ces zônes actives, dans ce cas, la présentation de ces taches par rapport à la Terre (the earthward presentation of such spots) risque moins d'être accompagnée de variations dans la réception.

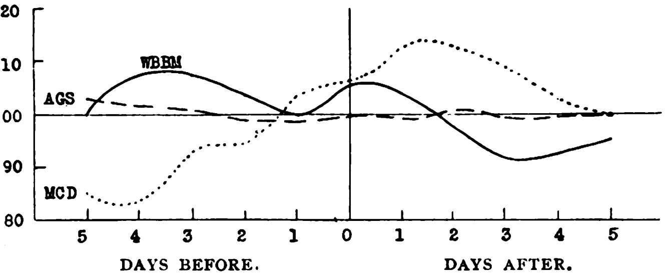

Fig. 1. — Intensité de la réception et champ magnétique pour des maxima d'au moins 4 taches dans une bande centrale de 13.3°. 88 cas, du 17 février 1926 au 21 mai 1927.

Comme lès zones actives persistent pendant un certain nombre de rotations solaires (parfois durant un an), comme, d'autre part, elles sont les sièges les plus probables de la formation des taches, nous pouvons continuer à utiliser l'aire et la position des taches pour établir des relations générales; cela, en dépit des cas individuels où les corrélations seraient en défaut (despite their inadaquacy in the individual case). Le but de ce mémoire est de montrer la non simultanéité de l'instant du passage des taches solaires au méridien central de l'astre et de l'instant auquel se produisent les troubles correspondants de la réception. On établira encore que l'intervalle de temps écoulé entre ces deux instants varie à la fois avec l'aire du groupe de taches et la période particulière choisie pour les observations.

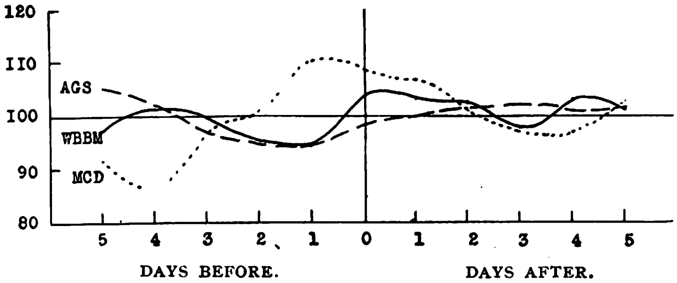

Fig. 2. — Intensité de la réception et champ magnétique pour des maxima de 4 à 10 taches dans une bande centrale de 13.3°. 62 cas, du 17 février 1926 au 21 mai 1927.

Dans la première des sept figures, on compare l'intensité de la réception nocturne, de la réception diurne, et du champ magnétique terrestre avec l'instant du passage des maxima de taches solaires au méridien central. Dans mes mesures, la réception nocturne s'effectue à Newton Center (Massachusetts), et Seabrook

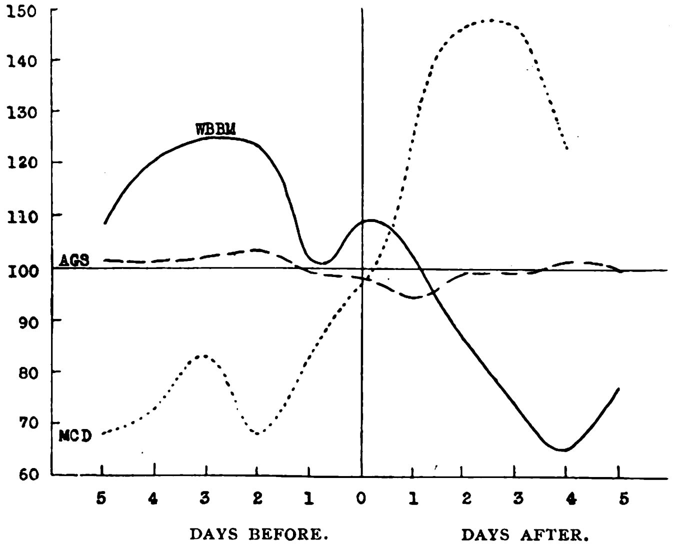

Fig. 3. — Intensité de la réception et champ magnétique pour des maxima d'au moins 12 taches dans une bande centrale de 13.3°. 26 cas, du 13 mai 1926 au 12 mai 1927.

Beach (New-Hampshire), de la station WBBM de Chicago; la réception diurne s'effectue à Washington de AGS de Nauen (Allemagne). La mesure magnétique adoptée ici est le « Magnetic Character of Day Number », donné par l'Observatoire de Cheltenham (Maryland). Les taches solaires furent comptées dans une bande centrale large de 13°3, ce qui représente le déplacement correspondant à un jour dans la rotation solaire (which is one day's travel in solar rotation).

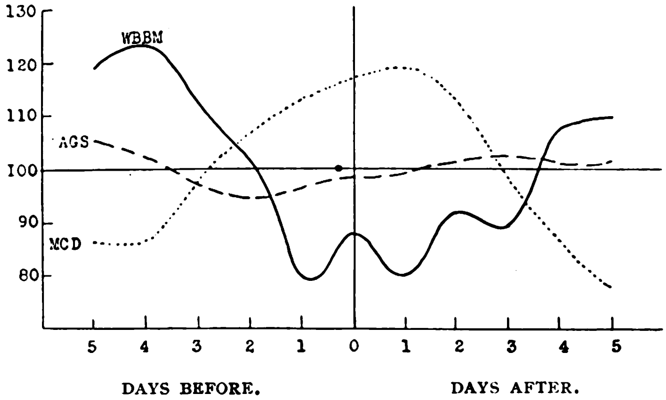

FIG. 4. — Intensité de la réception et champ magnétique pour des maxima de 4 à 10 taches dans une bande centrale de 13.3°. 26 cas, du 17 février 1926 au 4 octobre 1926.

La figure 1 montre la relation entre l'intensité des réceptions diurne et nocturne, du magnétisme terrestre, et l'instant de passage des maxima de quatre taches ou plus, pour la période qui va du 17 février 1927 au 21 mai 1927. Les ordonnées représentent la variation en pour cent des éléments, et le graphique couvre un intervalle de onze jours. La variation de la réception nocturne, représentée en trait plein, atteint un maximum à peu près trois jours après l'instant du passage des taches au méridien central. La variation de la réception diurne atteint un minimum mal défini un jour plus tôt. La variation magnétique, tracée en pointillé, présente un maximum qui se place dans la période de deux à trois jours qui suit l'instant du passage des taches.

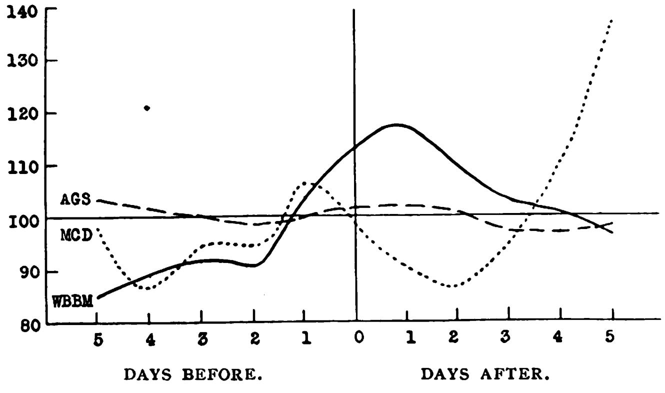

FIG. 5. — Intensité de la réception et champ magnétique pour des maxima de 4 à 10 taches dans une bande centrale de 13.3°. 36 cas, du 8 octobre 1926 au 21 mai 1927.

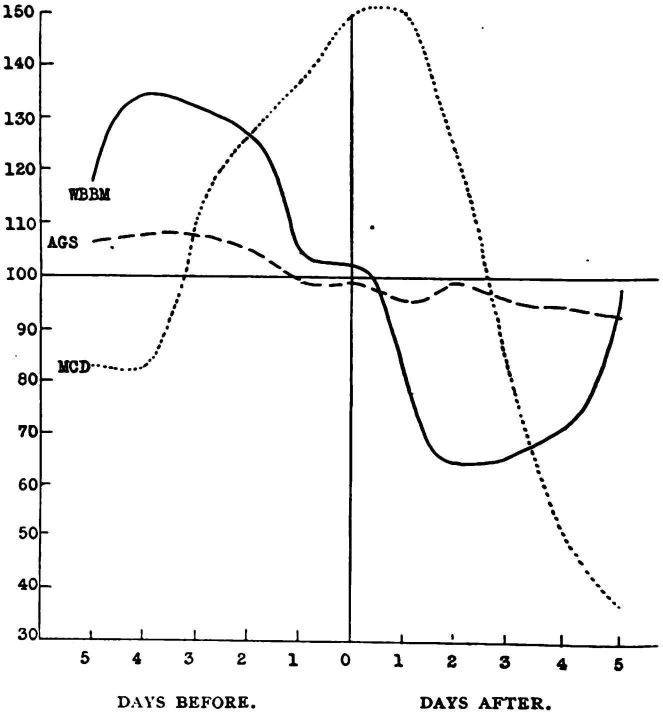

FIG. 6. — Intensité de la réception et champ magnétique pour des maxima d'au moins 12 taches dans une bande centrale de 13.3°. 8 cas, du 13 mai 1926 au 19 septembre 1926.

La figure 2 donne le résultat d'une comparaison entre l'intensité de la réception et du champ magnétique, et l'instant de passage des maxima de taches solaires limités cette fois de quatre à dix taches. A présent, il y a un minimum de l'intensité de la réception, tant diurne que nocturne, un ou deux jours avant l'instant du passage, et un maximum de la perturbation magnétique un jour avant.

On s'est borné, à la figure 3, à considérer des groupes de taches comprenant douze taches ou plus, dans la bande centrale. Cette fois la variation de la réception nocturne et celle de la perturbation magnétique ont augmenté; elles atteignent respectivement une valeur minimum et une valeur maximum, quatre et trois jours après l'instant du passage des taches. L'intensité de la réception diurne présente un faible abaissement un jour après.

Il résulte évidemment de ces graphiques que l'intervalle de temps écoulé entre l'instant du passage des taches solaires et celui de la diminution de l'intensité de la réception, est lié à la surface des taches solaires, comme si les groupes de taches solaires plus petits étaient vers la limite Ouest et les groupes plus grands vers la limite Est des zones actives. Mais il y a un autre facteur dont il faut tenir compte : la période recouverte par les observations. Dans les graphiques précédents, cette période a été d'environ quinze mois. Dans ce qui va suivre cet intervalle sera divisé en deux parties à peu près égales. Dans la figure 4, la période recouverte va du 17 février au 4 octobre 1926 : elle porte sur un maximum de taches solaires de quatre à dix. L'intensité de la réception nocturne est minimum un jour après l'instant du passage, la variation magnétique est maximum un jour après, et l'intensité de la réception diurne est minimum deux jours avant.

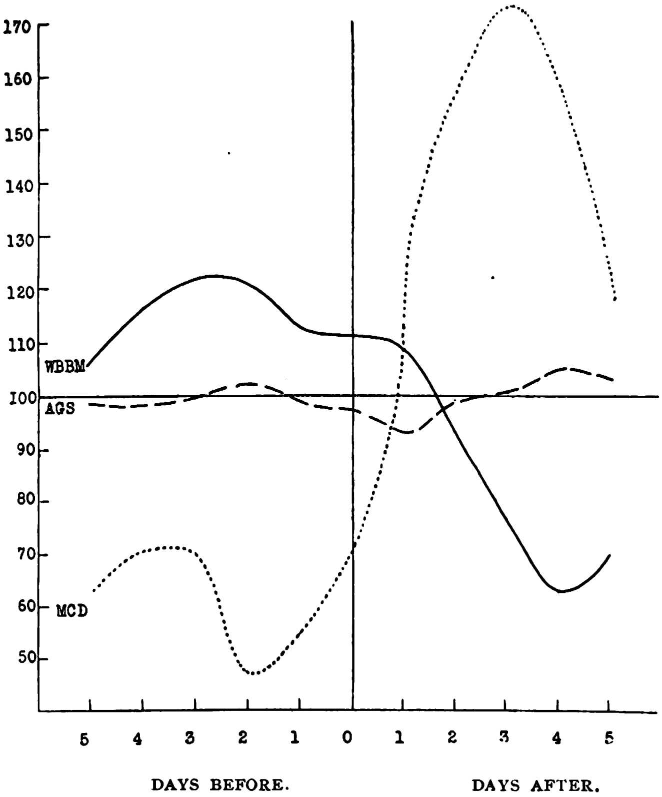

Fig. 7. — Intensité de la réception et champ magnétique pour des maxima d'au moins 12 taches dans une bande centrale de 13.3°. 18 cas, du 11 octobre 1926 au 12 mai 1927.

La période recouverte par la figure 5 va du 8 octobre 1926 au 21 mai 1927 : elle porte sur des maxima de taches de quatre à dix. A première vue l'allure des courbes de ce graphique paraît renversée, mais le maximum de l'intensité de la réception nocturne que l'on constate un jour après l'instant du passage des taches n'est pas un effet de ce passage. C'est seulement l'effet du retour de l'intensité à sa valeur normale, après quelque trouble antérieur. Ce qui s'est passé ici n'est qu'un grand décalage dans le temps. Le rapport renversé de la variation magnétique et de la variation de la réception nocturne est ici le meilleur guide pour trouver la véritable position du minimum de l'intensité de la réception : ce dernier est postérieur de plus de cinq jours à l'instant du passage de la tache. Apparemment, les groupes de taches plus petits étaient tout près du centre des zones actives, pendant les sept premiers mois; mais,

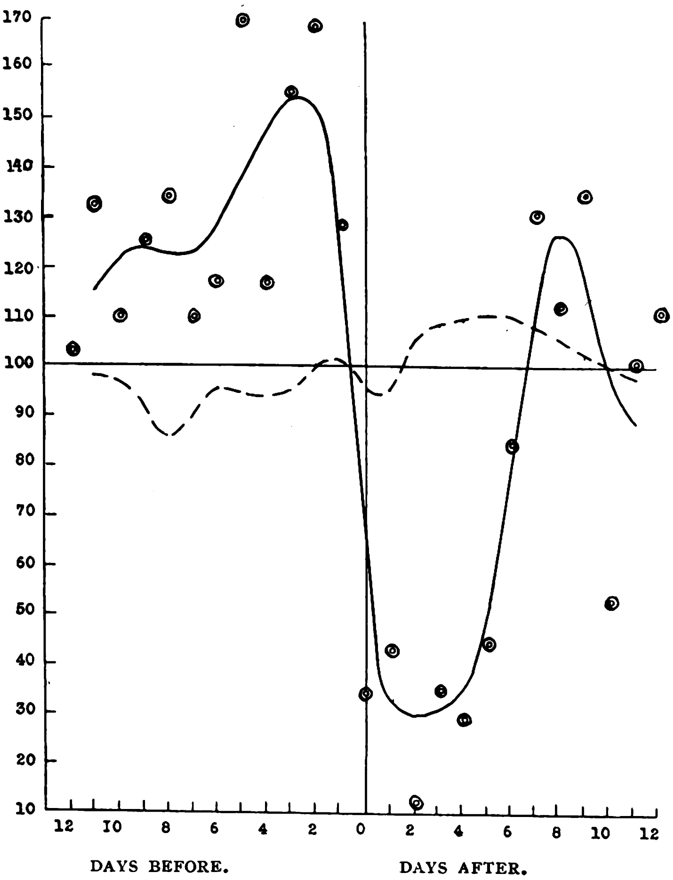

Fig. 8. — Intensité de la réception de WBBM et WGN lors d'orages magnétiques observés à Cheltenham. 14 cas, du 26 janvier 1926 au 19 août 1927. — Intensité de la réception diurne de AGS en traits interrompus.

pendant la seconde période, ils se déplaçaient vers l'Est d'au moins 65°.

Les groupes plus grands de taches présentent un décalage semblable, mais à un degré moindre. Dans la figure 6, l'intensité de la réception et la variation magnétique sont indiqués en fonction de l'instant des maxima de taches solaires groupées par douze ou plus, du 13 mai 1926 au 19 septembre 1926. L'intensité de la réception nocturne est minimum deux jours après, et la variation magnétique est maximum un jour après l'instant du passage des taches. La figure 7 se rapporte aussi à des maxima de taches de douze et plus, pour la période du 11 octobre 1926 au 12 mai 1927. Ici l'intensité de la réception et la variation magnétique sont respectivement minimum et maximum, trois et quatre jours après l'instant du passage. Le décalage est d'environ deux jours vers la droite.

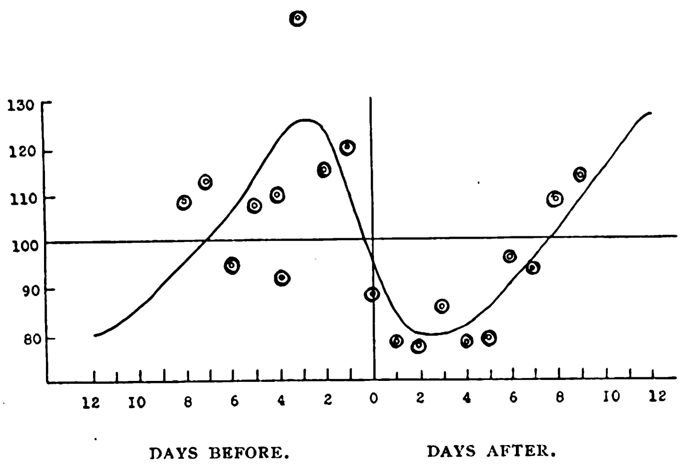

Fig. 9. — Intensité moyenne de la réception de WBBM et WJAX, à Washington et à Newton Centre, et maxima de perturbation magnétique, à Cheltenham. 36 cas, du 24 janvier au 12 juin 1927.

Il ressort de tous ces graphiques que la relation entre l'intensité de la réception et la valeur du champ magnétique est plus intime que la relation entre la valeur de cette intensité et l'instant du passage des taches solaires. Dans la figure 8, on a choisi comme événement l'orage magnétique au lieu des taches solaires. Durant les dix-neuf mois compris entre janvier 1926 et août 1927, il y a eu quatorze orages magnétiques enregistrés par Cheltenham. La diminution de l'intensité de la réception nocturne est très frappante. L'intensité de la réception diurne, au contraire, augmente pendant et après l'orage. Le maximum de l'intensité de la réception trois jours avant n'est pas un signe précurseur de l'orage qui arrive : c'est le retour à la valeur normale abaissée par une perturbation magnétique antérieure.

Bien que les troubles les plus marqués de la réception accompagnent des orages magnétiques, il peut se faire qu'une perturbation se manifeste malgré tout dans la réception pendant une journée peu troublée. Dans la figure 9, qui recouvre la période comprise entre le 24 janvier et le 12 juin 1927, on considère tous les jours à perturbations magnétiques, c'est-à-dire tous les jours dont le nombre caractéristique est de un ou deux. Les valeurs de l'intensité de la réception nocturne sont ici des moyennes arithmétiques de WBBM à Washington et Newton Center, et WJAX de Jacksonville (Floride), à Washington. Pendant la première moitié de 1927, des perturbations magnétiques se produisirent à intervalles d'environ quinze jours. Ainsi, le maximum de l'intensité de la réception que l'on constate trois jours avant, représente le retour à la valeur normale après un abaissement qui résulte d'une perturbation magnétique antérieure, et la courbe a une période d'environ quinze jours.

Les 19 et 20 août 1927, eut lieu un orage magnétique d'intensité considérable, avec production d'aurores polaires les deux soirs, visibles par toute la Nouvelle-Angleterre. Dans la figure 10, j'ai mis en évidence la diminution de l'intensité de la réception associée à cet orage, à la suite duquel le champ de WGN conserva durant une semaine une valeur réduite.

Pendant toute période étendue, l'intensité de la réception présente une grande série de dépressions brusques suivies de retours relativements lents à la valeur normale. Ce retour, au moins pendant un maximum de taches solaires, est rarement achevé avant la dépression suivante, en sorte que le champ moyen pour un mois, par exemple, est déterminé d'une manière très nette par l'intervalle entre ces dépressions brusques. Si cet intevalle est court, par exemple de l'ordre d'une semaine, le champ moyen sera faible;

s'il est de l'ordre de deux ou trois semaines consécutives, le champ moyen sera élevé. Il semble définitivement établi que ces diminutions accompagnent des perturbations du magnétisme terrestre, et, moins définitivement, qu'elles soient liées à l'instant du passage des groupes de taches solaires au méridien central de l'astre. Néanmoins, dans aucun cas nous ne pouvons affirmer qu'il existe un rapport de cause à effet. Le rapport entre l'intensité de la réception et la valeur du magnétisme terrestre est cependant si étroit qu'on ne peut que difficilement s'empêcher de leur attribuer une cause commune, à savoir, une variation dans l'état électrique de l'atmosphère. Tout désaccord dans cette relation pourrait s'expliquer en supposant que les perturbations magnétiques exigent un mouvement électrique, tandis que l'intensité de la réception dépendrait du degré et de la répartition de l'ionisation.

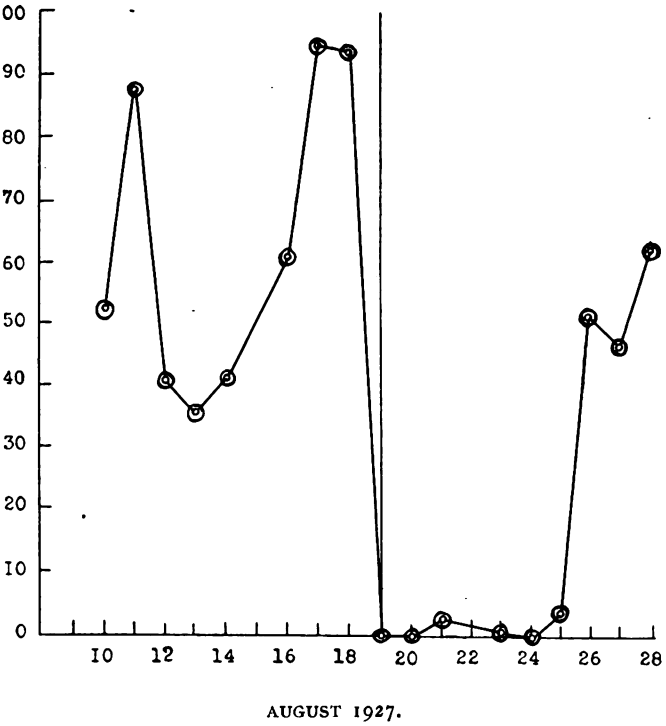

FIG. 10. — Intensité de la réception nocturne de WGN de Chicago, à Seabrook Beach, N. H., et l'orage magnétique des 19 et 20 août 1927.

* * *

Variations nocturnes apparentes décélées par les radiophares à bobines croisées,

par HARADEN PRATT,
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Il apparut de bonne heure que l'emploi en avion, de cadres, pour déterminer les directions ou les emplacements de stations radiotélégraphiques, comportait des difficultés. Les dimensions des bobines étaient limitées et il fallait effectuer une amplification considérable pour capter un signal d'intensité convenable. Les interférences, dues à l'allumage du moteur d'avion, dans le système récepteur, empêchaient d'utiliser une forte amplification et, ainsi, la portée était limitée. En outre, le bruit intense qui existe à bord de l'avion empêchait d'observer d'une manière précise le secteur d'extinction pendant la rotation de la bobine, compromettant de ce fait l'exactitude de cette méthode lorsqu'on l'applique à la navigation aérienne.

On essaya divers moyens pour repérer d'un avion, la position des stations radiotélégraphiques sans trouver de solution pratique à ce problème, et ce n'est que lorsque le radiophare à bobines croisées fut proposé que ceux qui s'intéressaient à cette technique furent encouragés à attendre l'évolution d'un système simple et maniable pour diriger l'avion.

L'idée du phare à bobines croisées fut étudiée par le « Bureau of Standards » il y a quelques années; depuis ce moment, son développement a été rapide et il en existe à présent plusieurs en service dans la partie orientale des Etats-Unis.

Ce phare comprend essentiellement deux cadres disposés dans deux plans verticaux fixés entre eux en faisant un angle. Ce système de deux bobines peut tourner librement autour d'un axe vertical. Quand les bobines sont excitées de la même manière en haute fréquence, les signaux d'égale intensité de chacune seront perçus dans un montage de réception, pour autant qu'il soit situé dans un des quatre plans verticaux bissecteurs des angles que font entre eux les plans des bobines. En d'autres endroits, les intensités des signaux de chaque bobine seront différentes. Ce tracé dans l'espace d'un plan vertical d'égale intensité des signaux, constitue la caractéristique de direction du type de phare à bobines croisées, et cette zone d'égale intensité de signaux est souvent mentionnée comme la route qu'il trace.

Dans son application pratique et pour des motifs de commodité dans la construction, le phare employé dans ce pays utilise comme système rayonnant deux grands cadres, à un seul tour, fixes. Un goniomètre approprié, interposé entre ces cadres et la source de puissance, permet, par sa rotation, la rotation dans l'espace des zones d'égale intensité de signaux. Un dispositif mécanique transmet automatiquement la lettre A sur une bobine et la lettre N sur l'autre. Celles-ci sont émises de telle façon qu'on perçoit un trait continu dans toute la zone d'égale intensité de signaux. La largeur de cette zone, où l'on ne perçoit que le trait, dépend en grande partie de l'acuité d'audition de l'observateur, et peut varier entre des limites de 1,5 à 3 degrés, par exemple.

Le phare à bobines croisées possède plusieurs avantages manifestes pour la direction des avions.

1. Il ne faut pas observer de zone de maximum ni de minimum d'intensité de signaux.
2. La position de la route ou du rayon est donnée, par une comparaison automatique de deux signaux.
3. Quelle que soit la situation de l'avion dans un large espace angulaire, quand il s'écarte de sa route, le phare fournit un signal déterminé permettant à l'avion de se situer, et de retourner vers sa bonne route. Cette caractéristique permet de faire des détours momentanés, pendant le vol, dans des zones orageuses ou obstruées, chose très importante et très précieuse.
4. L'avion utilise un dispositif récepteur ordinaire, muni de l'antenne à fil postérieure, permettant de recevoir les signaux dans les conditions plus favorables au point de vue de la sensibilité minimum du poste récepteur d'avion aux bruits et perturbations locales.
5. Un avion peut être dirigé le long d'une voie aérienne tracée et invariable indépendamment de la dérive due au vent.

C'est à ces avantages qu'est dû le choix de ce type de phare pour l'application à notre système aérien national, particulièrement pour les lignes de navigation commerciale.

A titre d'information pratique, on a installé récemment un montage récepteur sur un avion de trafic voyageant entre les aéroports de Cleveland et de New-York, sur un parcours aérien de 380 milles environ. Il y avait deux phares directeurs, l'un au terminus de New-York et l'autre à un endroit à 170 milles à l'ouest, à Bellefonte, Pa. La fréquence employée était 290 kilocycles. Les cadres rayonnants étaient de forme triangulaire, de 300 pieds de longueur et 80 pieds de hauteur. Le courant dans chaque cadre était de 8 ampères. Cette route traverse la chaîne des monts Alleghany qui occupent la presque totalité de cette région et est particulièrement propice pour ces vols expérimentaux, tant à cause de la topographie abrupte qu'à cause du temps dominant, brumeux et nuageux.

On avait déjà établi par l'expérience du passé, que la précision du phare directeur utilisé comme guide, était de premier ordre, mais en examinant l'état de la question, on s'aperçut que toute expérience antérieure effectuée avec ce phare se réduisait à un vol diurne. On n'avait pas éprouvé son fonctionnement pendant la nuit. On entreprit à cet effet, l'été dernier, des vols sur la ligne aérienne mentionnée ci-dessus et l'on observa aussitôt des résultats incompatibles avec ceux obtenus pendant le jour. Durant le premier vol, il sembla impossible de maintenir l'avion sur une route correspondant au signal à trait combiné. A peine le signal était-il reçu qu'il se muait en lettre A ou en lettre N. Aucune manœuvre de l'avion ne pouvait améliorer la situation. Le signal trait apparaissait et disparaissait à intervalles de quelques minutes. On comprit bientôt qu'à cette distance de l'ordre de 90 milles, il n'existait aucune route tracée et que la zone d'égale intensité de signaux se déplaçait d'une manière non définie.

Cet effet déconcertant montrait la nécessité d'étudier le phénomène, et plusieurs vols nocturnes jusqu'à des distances de 175 milles du phare furent entrepris. Dans chaque cas le changement de la zone ou de la route était noté. Les résultats généraux déduits de cinq vols effectués à une altitude moyenne de 2,000 pieds peuvent s'énoncer comme suit :

1. Dans un rayon de 25 milles autour du phare, la variation n'était pas de nature très importante.
2. A 50 milles la variations devenait prononcée mais grâce à la zone qui était stationnaire dans sa position exacte pendant les 75 % du temps environ, le phare pouvait encore servir.
3. A une distance de 100 milles, la variation devenait très prononcée et persistait pendant plus de 50 % du temps.
4. A 125 milles le phare n'avait plus aucune valeur comme guide.
5. La variation de la zone était continue de telle sorte qu'à première vue, on pouvait penser qu'elle était due au mouvement de l'avion.
6. Il apparut que la topographie de la région entre le phare et l'avion jouait un rôle considérable quant à l'importance de la variation.
7. On nota des écarts exceptionnels dans la variation, de l'ordre de 100 degrés (as much as 100 degrees) en azimut, mais, en général, la variation restait dans les limites de 25 degrés.
8. Au delà de 15 milles, l'évanouissement des signaux reçus était considérable lorsque l'avion survolait des montagnes. Comme il y a plusieurs variables, on n'est arrivé à aucune conclusion quant à l'influence relative de chacun des facteurs, sinon qu'on observait des signaux plus constants en des points déterminés sur le sol qu'en l'air, à des distances égales du phare.

On a fait pendant la nuit, quelques observations sur terre du phare de Bellefonte à Washington. Quand le phénomène de variation se manifestait, il était moins prononcé que celui qu'on observait en l'air. Lors d'un vol nocturne de Harrisburg à Washington, on n'observa aucune variation de la zone, en utilisant les signaux d'un phare de College Park, Md Comme il n'y a pas de chaîne de montagnes marquée près de College Park, ces observations indiqueraient que des caractères topographiques ont une influence importante sur la nature de ces variations.

Pour éclaircir davantage la question, on a fait récemment deux séries de mesures nocturnes des signaux du phare de Bellefonte, l'une à un point distant de 22 milles et l'autre de 32 milles, les deux emplacements étant en région montagneuse. On enregistra des signaux avec un enregistreur graphique des intensités de champ, en utilisant à la fois une antenne verticale et une bobine placée dans un plan vertical s'étendant vers le phare. Les rapports observés de la variabilité à l'intensité moyenne du champ sont :

à 22 milles,	avec bobine.	0.25
	avec antenne verticale .	0.00
à 32 milles,	avec bobine.	0.44
	avec antenne verticale .	0.07

Les rapports observés de la variabilité à l'intensité maximum du champ sont :

à 22 milles,	avec bobine.	0.43
	avec antenne verticale .	0.05
à 32 milles,	avec bobine.	0.56
	avec antenne verticale .	0.1

En faisant tourner la bobine autour de son axe vertical de façon à recevoir le signal minimum, on mit en évidence une variation dans la direction du champ incident, s'étendant sur 30°, dans un intervalle de dix minutes.

En terminant, je dois mentionner que bien que la zone d'égale intensité de signaux reste ordinairement stationnaire dans sa position correcte pendant les heures de la journée, on a noté de brusques variations durant quelques moments, dans les régions montagneuses, surtout quand on survole des vallées à des altitudes de 1,000 à 1,500 pieds.

Nous espérons que ce court résumé des quelques observations préliminaires faites par le personnel du « Bureau of Standards », servira à attirer l'attention sur la réalisation de ce type de radiophare de façon à ce que les limites de son utilisation comme guide pour les vols de nuit puissent être étudiées, et que l'on acquière des données pratiques qui permettent de mieux comprendre les caractéristiques d'emploi de ce système et les conditions qui exercent une influence.

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Un oscillateur à haute fréquence pour les recherches relatives à la réception,

par George RODWIN et Théodore A. SMITH,
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(New-York, U. S. A.)

Un oscillateur à haute fréquence, modulé, constitue un appareil extrêmement utile dans un laboratoire expérimental. En lui adjoignant un certain nombre de dispositifs (features) spéciaux, un tel oscillateur peut acquérir des usages multiples et ses applications peuvent devenir très variées.

Le montage que nous allons décrire fut primitivement construit en vue de travaux effectués pour déterminer certaines caractéristiques de radiorécepteurs (1).

Cet oscillateur a encore servi à produire des champs connus pour la mesure des intensités de champs, à mesurer des résistances et des inductances de bobines, à équiper (to lining up) des circuits radioélectriques; il a servi encore à divers autres travaux de laboratoire.

L'appareil projeté devait satisfaire autant que possible aux conditions suivantes :

1. La puissance de l'oscillateur devait être largement variable sans modification appréciable de la fréquence.
2. L'appareil devait être muni d'un dispositif permettant d'évaluer la puissance.
3. La fréquence devait être variable sur une étendue donnée (telle l'échelle de la radiodiffusion), l'appareil restant calibré.
4. Pour chaque fréquence fixée, il devait être possible de faire varier la fréquence par degrés insensibles de part et d'autre de la valeur fixée avec un grand degré de précision.
5. On devait pouvoir obtenir la modulation jusqu'à 50 % au moins en restant à même d'indiquer continuellement le pourcentage de la modulation.
6. Le champ de force (stray field) émanant de l'appareil devait être faible par rapport au champ produit par le dispositif calibré de rayonnement employé.
7. Les accessoires mécaniques devaient permettre à l'opérateur de faire les lectures rapidement et facilement.

L'appareil que nous allons décrire satisfait à ces conditions. Beaucoup d'accessoires ont été profondément renforcés et conservés dans cette invention; il en résulte que l'oscillateur paraît plutôt grand et massif. Cependant si l'on songe à la variété de ses usages, sa grande dimension semble justifiée.

CONSTRUCTION ÉLECTRIQUE.

L'appareil complet consiste en un oscillateur modulé, amplificateur de puissance avec un circuit de débit et de mesure. La figure 1 en représente un schéma.

Circuits généraux. — On a trouvé que la méthode d'amplification de la puissance d'un oscillateur fondamental (master-oscillator-power amplifier), pour la génération de courants de haute fréquence, présentait plusieurs avantages par rapport à l'emploi d'un simple oscillateur. L'oscillateur utilise une lampe UX — 201A dans un circuit Hartley ordinaire. Un condensateur de grille de 500 micro-microfarads avec un filtre de grille de 20,000 ohms rendent la fréquence suffisamment constante pour de légères variations dans les tensions de filament et de plaque, sans réduire beaucoup le débit de la haute fréquence. Pour l'échelle de la radiodiffusion, une self-induction de 82 micro-henrys constitue, avec un condensateur variable d'une capacité maximum de 1,000 micro-microfarads, le circuit accordé. Le condensateur variable, qui est du type linéaire, est construit expressément grand, de telle sorte que la partie réellement rectiligne de son étalonnage de fréquences est seule utilisée en

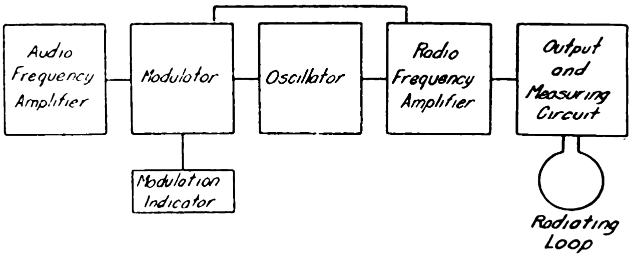
Audio Frequency Amplifier
Modulator
Oscillator
Radio Frequency Amplifier
Output and Measuring Circuit
Modulation Indicator
Radiating Loop

Fig. 1. — Schéma de l'oscillateur-amplificateur modulé.

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(1) *Proceedings of the Institute of Radio Engineers*, vol. 15, n° 5, May, 1927, p. 389.

temps normal tout en permettant des variations de fréquence de un à trois. Afin de réduire au minimum l'effet de la capacité propre des diverses lampes sur l'étalonnage de la fréquence, on place un petit condensateur fixe d'environ 60 micro-microfarads en parallèle avec le condensateur principal d'accord. On trouva que cette addition améliorait la courbe d'étalonnage en fréquences de la graduation principale.

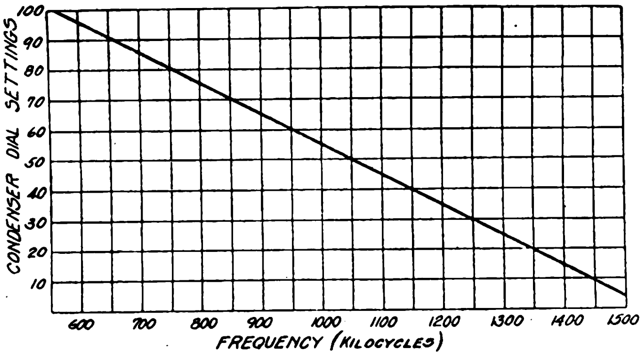

FIG. 2. — Fréquence de l'appareil. — Etalonnage.

La figure 2 représente cette courbe pour l'appareil construit.

On emploie un dispositif mécanique pour faire varier la fréquence par petites quantités. Ce dispositif est décrit en détail ci-dessous. On peut obtenir des fréquences en échelons de 1,000 à 8,000 cycles de part et d'autre d'une fréquence donnée. Ce dispositif fut étalonné comme suit :

Le vernier étant à son zéro, on produit un battement nul dans un radiorécepteur à l'aide d'un oscillateur extérieur séparé, dont on est assuré que la fréquence reste constante pendant le temps de l'étalonnage. Quand le vernier est écarté de sa position de zéro, une note de battement va se produire, que l'on compare à une source de fréquence acoustique calibrée, en réglant le son de la note de battement de façon à ce que sa fréquence soit égale à la fréquence acoustique connue. On a répété cette opération pour un certain nombre de fréquences allant jusqu'à 8,000 cycles de part et d'autre de la position de zéro du vernier.

Comme le vernier est construit de manière à produire un déplacement angulaire constant du condensateur d'accord pour chaque rotation donnée du vernier, et comme le condensateur variable d'accord a une caractéristique de fréquence linéaire (straight line), le calibrage du vernier sera conservé pour toutes les fréquences. Cela a été vérifié en répétant les comparaisons de la note de battement, décrites ci-dessus, pour de multiples positions du condensateur et l'on a trouvé que le calibrage du vernier était pratiquement constant sur toute l'étendue des fréquences.

L'amplificateur de puissance haute-fréquence a son circuit de grille couplé au circuit oscillant par l'intermédiaire d'une inductance non accordée. Deux lampes UX — 112 sont employées en parallèle et elles sont neutralisées. L'amplificateur de puissance fonctionne uniquement comme amplificateur et son excitation de grille est telle qu'on obtient un débit maximum dans ces conditions. Le circuit de plaque de l'amplificateur est accordé par un condensateur placé sur l'axe du condensateur principal d'accord.

Le circuit d'émission est couplé à la bobine de plaque de l'amplificateur, son couplage étant constamment variable en vue de faire varier l'émission.

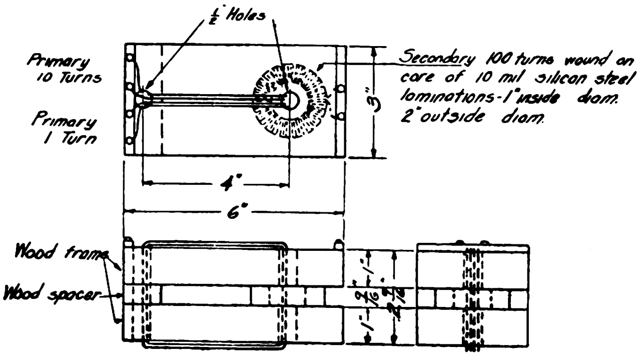
½" Holes
Primary 10 Turns
Primary 1 Turn
Secondary 100 turns wound on core of 10 mil silicon steel laminations-1" inside diam. 2" outside diam.
3"
4"
6"
Wood frame
Wood spacer
1"
1"
1/16"
2 1/16"

FIG. 3. — Transformateur de courant pour la mesure de débits.

On a trouvé désirable d'avoir une grande étendue de variation de débit — au moins 5,000 à 1, quant au courant. La combinaison d'un transformateur pour courant de haute fréquence, d'un milliampèremètre à couple thermoélectrique et d'un dispositif de couplage a donné satisfaction pour fournir et mesurer un débit aussi variable.

Transformateur de courants pour la production de débits variés. — Le transformateur de courants pour les hautes fréquences (1) tel qu'il est employé dans cet appareil, possède un noyau métallique en forme de tore composé de tôles en ferro-silicium de 0cm0254 ; son épaisseur est de 1cm43 ; son diamètre extérieur de 5cm08 ; son diamètre intérieur de 2cm54. Autour du noyau qui est recouvert de soie huilée isolante, se trouve un enroulement de 100 tours, régulièrement espacé sur ce noyau. L'autre enroulement soit de 1, soit de 10 tours, passe à travers le trou du tore. Des blocs de bois maintiennent les enroulements bien espacés l'un de l'autre de manière à diminuer les effets de capacité entre ces enroulements et à conserver le rapport des intensités des deux courants (current ratio). La construction est représentée sur la figure 3.

On peut démontrer que le rapport des intensités des courants, pour un semblable transformateur, est donné par une expression de la forme

$$\frac{I_1}{I_2} = \frac{n_2}{n_1}\left(1 + \frac{\alpha R_2}{\omega L_2}\right)$$

où I_1 et n_1 sont respectivement les valeurs de l'intensité du courant et du nombre de tours de l'enroulement primaire.

I_2 et n_2 sont respectivement les valeurs de l'intensité du courant et du nombre de tours de l'enroulement secondaire.

R_2 et L_2 sont respectivement la résistance et la self-induction du secondaire.

α est un coefficient dont la valeur est généralement inférieure à 1 et qui dépend des pertes dans le fer.

Quand $\frac{\alpha R_2}{\omega L_2}$ est petit devant l'unité, la relation précédente se réduit à

$$\frac{I_1}{I_2} = \frac{n_2}{n_1}.$$

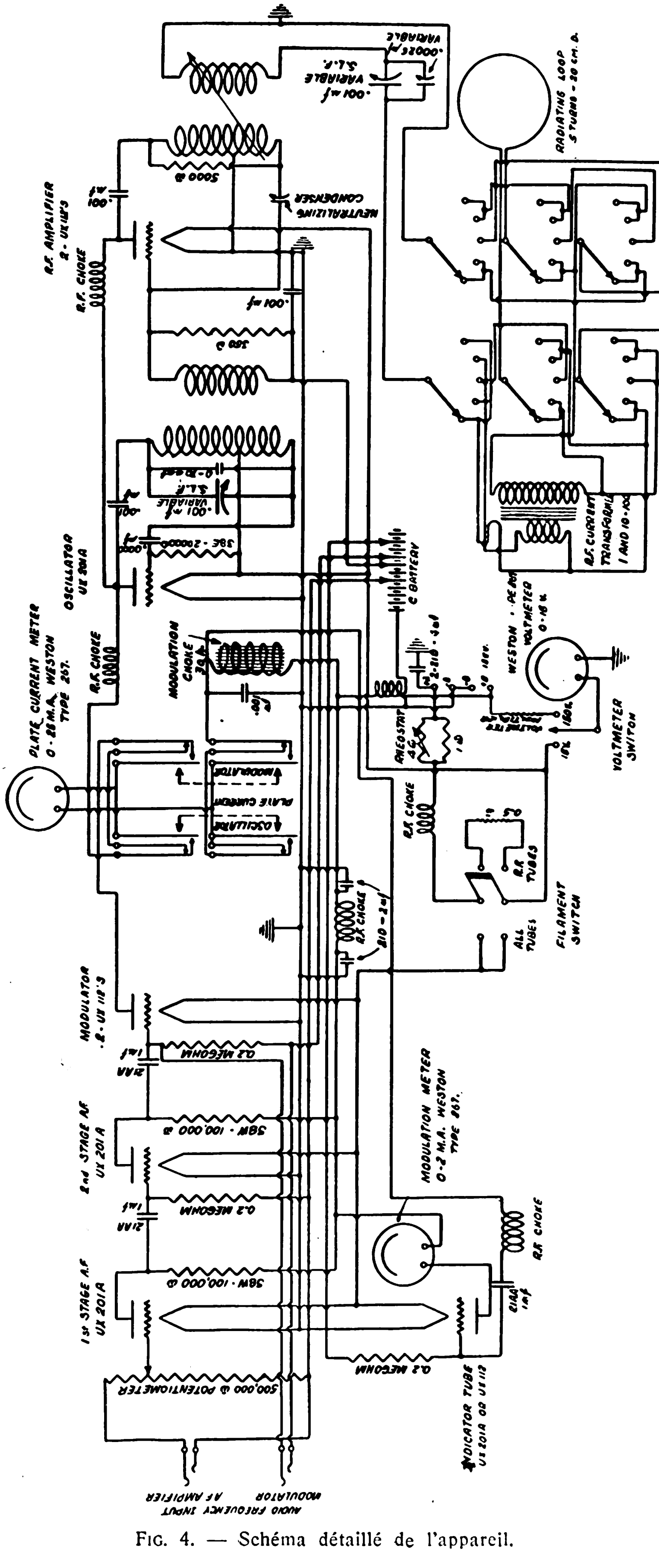
R.F. AMPLIFIER 2-UX112
R.F. CHOKE
NEUTRALIZING CONDENSER
OSCILLATOR UX 210A
PLATE CURRENT METER 0-25 M.A. WESTON TYPE 267
R.F. CHOKE
MODULATION CHOKE
MODULATOR 2-UX 112
0.2 MEGOHM
2nd STAGE AF UX 201A
1st STAGE A.F UX 201A
500,000 Ω POTENTIOMETER
AUDIO FREQUENCY INPUT
AF AMPLIFIER
MODULATOR
RADIATING LOOP
THERMO-MILLIAMMETER
RADIO FREQUENCY RATIO SWITCH
R.F. CURRENT TRANSFORMER
C BATTERY
RHEOSTAT
R.F. CHOKE
VOLTMETER SWITCH
FILAMENT SWITCH
ALL TUBES
R.F. TUBES
MODULATION METER 0-2 M.A. WESTON TYPE 267
R.F. CHOKE
INDICATOR TUBE UX 201A OR UX 112

Fig. 4. — Schéma détaillé de l'appareil.

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(1) D. W. Dye, *Measurement of Alternating Electric Currents of High Frequency.* (Proceedings of Royal Society [of Great Britain] A-1914, vol. XC, p. 621.)

Bureau of Standards Circular, 74, p. 153.

D. W. Dye, *Current Transformer Methods of Producing Small Voltages and Currents at Radio Frequencies for Calibrating Purposes.* (Journal Institution of Electrical Engineers, London, June 1925, vol. LXIII, p. 597.)

Le circuit secondaire comprend la résistance de l'ampèremètre; pour que le rapport de transformation se conserve, cette résistance doit être rendue petite devant la réactance (inductive reactance) du secondaire du transformateur. Pour le transformateur ayant les dimensions données, employé à des fréquences de la gamme de la radiodiffusion, un milliampèremètre thermique de 43 ohms (0.8 milliampères) a donné satisfaction. Le rapport était vérifié en mesurant les courants dans les deux enroulements et on obtint une approximation supérieure à 5 % dans l'étendue des fréquences et des courants imposés.

La figure 4 représente le schéma complet des connexions de l'appareil, et l'on peut y voir la façon d'utiliser le transformateur de courant. Un interrupteur sextipolaire à cinq bornes permet d'utiliser l'ampèremètre soit dans le primaire soit dans le secondaire du transformateur de courants de telle sorte que l'on puisse obtenir des rapports d'intensités de courants de 1/100, 1/10, 1/1, 10/1 ou 100/1 entre le circuit de l'ampèremètre et celui de charge ou circuit du cadre rayonnant. La capacité entre les diverses parties de l'interrupteur a été choisie faible. En outre, les effets dûs à toute capacité parasite sont réduits au minimum par l'emploi de circuits de charge et de mesure à faible impédance.

Production de champs connus. — Le cadre circulaire rayonnant parcouru par un courant connu peut servir à induire une tension aisément calculable dans un second cadre, l'induction mutuelle entre les deux cadres ayant une valeur connue. Les intensités du champ connu dans un plan situé à n centimètres de la spire et parallèle au plan de cette spire, peuvent être calculées à partir du champ H qui vaut

$$H = \frac{2\pi a^2 i n}{10} \times \frac{1}{(x^2 + a^2)^{3/2}} \text{ gauss.}$$

L'intensité du champ en $\frac{\text{volts}}{\text{cm}} = 300$ H.

Ainsi,

$$E = 18.850 \frac{a^2 in}{(x^2+a^2)^{3/2}} \times 10^6 \text{ microvolts par}$$

mètre où a est le rayon du cadre en centimètres.

i est le courant dans le cadre en ampères.

n est le nombre de tours de spires du cadre circulaire.

Si le plan où l'on désire connaître E fait un certain angle avec celui du cadre circulaire, l'intensité du champ peut être calculée à partir de l'expression précédente, en introduisant un facteur où intervient le cosinus de l'angle des deux plans. On obtient promptement de la sorte des intensités de champs s'étendant de 10 à 1,000,000 de microvolts par mètre.

Modulation. — On emploie pour la modulation, la méthode donnant sous le nom de « bobine de réaction dans le circuit de plaque » (plate circuit choke coil); deux lampes UX — 112 commandent à la fois le maître oscillateur (master oscillator) et l'amplificateur de puissance. On obtient de très hauts degrés de modulation. On a trouvé que la relation entre la tension d'alimentation (input voltage) et le pourcentage de la modulation est linéaire jusqu'à 70 %. Pour mesurer le pourcentage de la modulation, on ne peut pas appliquer la méthode ordinaire qui consiste à mesurer la tension de basse fréquence aux bornes de l'inducteur de modulation, et à comparer cette valeur à la tension du courant de plaque direct de l'oscillateur et du modulateur. Cela est dû au fait que l'oscillateur et l'amplificateur haute-fréquence doivent être tous deux modulés (are being modulated). On a utilisé l'oscillographe cathodique pour la détermination du pourcentage de la modulation. Une des armatures des plaques de l'oscillographe est excitée par la tension induite dans une bobine et élevant la tension (picking up) couplée au cadre rayonnant. L'oscillateur est modulé par une source à basse fréquence. L'autre armature des plaques de

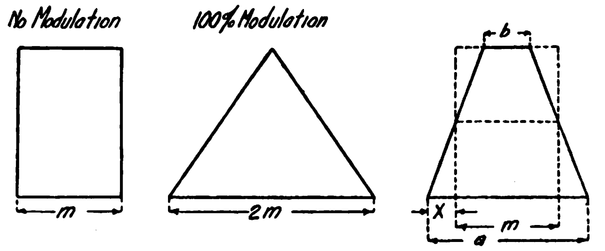

Fig. 5. — Figures de l'oscillographe cathodique utilisées dans la détermination du pourcentage de modulation.

l'oscillographe est alimentée en tension par la même source basse fréquence. En général, l'oscillogramme résultant est un trapèze. La figure 5 représente les diverses formes qu'il peut prendre.

En utilisant les symboles de la figure 5, la modulation est donnée par $\frac{2x}{m} \times 100\ \%$. Mais $m = \frac{a+b}{2}$ et $x = \frac{a-b}{4}$, donc la modulation $= \frac{a-b}{a+b} \times 100\ \%$.

A titre d'indication, on emploie une lampe UX — 201A dont le courant de plaque normal est réduit pratiquement à zéro et dont la grille est excitée par une tension basse-fréquence développée dans le dispositif de réaction de modulation. Le circuit de plaque de cette lampe renferme un milliampèremètre allant de 0 à 2, dont l'échelle a été étalonnée directement en pourcentages de modulation, en employant la méthode de l'oscillographe décrite plus haut. Un condensateur d'arrêt en haute fréquence empêche la tension de haute fréquence d'affecter la lecture fournie par la lampe indicatrice. Quand les lampes amplificatrices de haute fréquence opèrent convenablement et que les circuits sont équilibrés (equalized) l'indication de modulation (modulation indication) est pratiquement constante quand la haute fréquence varie.

Un amplificateur à deux étages, à couplage par résistance fournit une tension suffisante aux grilles modulatrices. Sa caractéristique est pratiquement unie sur une grande étendue de basses fréquences.

Régularisation du courant débité. — On avait observé que le courant débité de haute fréquence variait considérablement avec la fréquence, et on estimait désirable que le débit fût constant. On envisagea un moyen de remédier sensiblement à ce défaut. Il consistait à placer des résistances fixes adéquates, en série aussi bien avec la bobine de couplage de l'amplificateur haute fréquence qu'avec sa bobine de débit. La variation du courant débité dans le cadre rayonnant était, de la sorte, rendue inférieure à 15 %, pour un montage du couplage à débit constant, sur toute l'étendue des fréquences. On s'aperçut aussi que ces résistances amoindrissaient l'effet du circuit accordé sur les caractéristiques de basse fréquence.

La figure 6 représente les caractéristiques générales (overal) suivant le rapport de la tension basse fréquence à la tension modulée haute fréquence (from audio amplifier input to modulated radio frequency output). Les données de ces courbes furent prises en redressant une partie de la tension survoltée par un tube détecteur à grille négative et en observant le débit de basse fréquence. Il reste encore un léger effet de sélection du circuit de résonance, mais cela n'a pas une influence considérable, et peut être toléré même dans des mesures exactes.

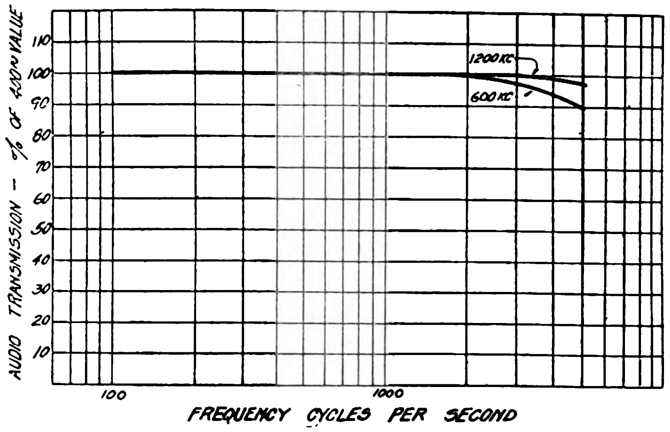

Fig. 6. — Caractéristiques de basse fréquence générales de l'appareil.

CONSTRUCTION MÉCANIQUE.

Dispositifs d'accord et de vernier. — Les condensateurs d'accord de l'oscillateur et du (circuit) de débit sont montés tous deux sur le même axe et sont commandés par un simple cadran sur le panneau de face. On a jugé bon d'employer comme système de vernier un dispositif à came de friction. Celui-ci consiste en un levier et une plaque de friction axés sur l'arbre principal et en une came axée sur un arbre de vernier séparé. L'extrémité du bras de levier s'appuie (bears on) sur la came et elle est maintenue en position par un ressort puissant. Tout mouvement de l'arbre du vernier obligera le levier à se mouvoir un peu soit vers le haut soit vers le bas et puisque la plaque de friction est maintenue solidaire de l'arbre principal, les condensateurs d'accord principaux bougeront légèrement. L'arbre principal peut être mû sans troubler le dispositif de vernier, puisque la plaque de friction glissera (will slip) et que le bras de levier du vernier restera immobile grâce à l'action combinée du ressort et du support de la came (cam rest). Le critérium pour savoir si un tel système sera efficace consiste en ceci : la force nécessaire pour déplacer la plaque de friction sur son support (bearing) doit être plus grande que le frottement des paliers dans les condensateurs d'accord. Il en est aisément ainsi dans la pratique. On trouvera les détails sur les figures 7 et 8.

Tout l'appareil est monté dans une boîte doublement garnie de cuivre. Le panneau de face et le tableau de base ont été rendus amovibles indépendamment pour la facilité. Les diverses pièces, telles que l'amplicateur basse fréquence, l'oscillateur, l'amplificateur haute fréquence et le circuit de débit, sont protégées les unes des autres. On peut en voir le détail sur les figures 9 et 10. Les batteries de grilles sont placées dans un compartiment séparé, mais la batterie des filaments de 6 volts et la batterie de plaque de 135 volts sont placées en dehors de la boîte recouverte. Des filtres haute fréquence dans les armatures des batteries empêchent le rayonnement de celles-ci.

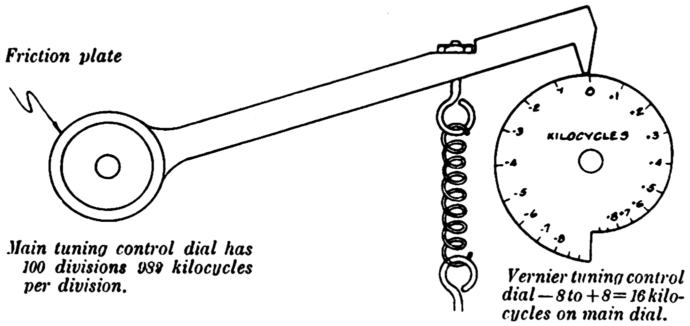
Friction plate
KILOCYCLES
0
.1
.2
.3
.4
.5
.6
.7
.8
.1
.2
.3
.4
.5
.6
.7
.8
Main tuning control dial has 100 divisions 989 kilocycles per division.
Vernier tuning control dial — 8 to + 8 = 16 kilocycles on main dial.

Fig. 7. — Came et levier utilisés pour le réglage du vernier.

Le milliampèremètre à couple thermoélectrique pour la mesure du débit est placé à l'extérieur au voisinage de l'appareil, une paire de conducteurs torsadés (twisted) étant employée pour les connexions.

Pour terminer, nous tenons à signaler que ce travail fut entrepris sous la direction de M. Julius Weinberger, dont les suggestions furent très précieuses dans le développement de l'installation.

RÉSUMÉ.

On décrit un oscillateur à haute fréquence modulé. Cet appareil permet d'obtenir des débits de haute fréquence d'une étendue largement variée, comporte un système de mesure, permet de faire varier la fréquence produite par degrés réduits de part et d'autre d'une fréquence donnée et comporte encore un système modulateur et indicateur.

* * *

L'effet de la réaction sur l'intensité du signal reçu (1),

par Balth. VAN DER POL, D. Sc.
(Eindhoven-Pays-Bas)

Tout le monde connaît l'augmentation considérable de l'intensité d'un signal que l'on peut obtenir en utilisant la *réaction*. On sait aussi que cette augmentation dans l'intensité du signal est beaucoup plus forte lorsque le signal incident est faible que lorsqu'il est fort. Le but de ce mémoire est d'établir une théorie non linéaire de cet effet de réaction et d'en donner les vérifications expérimentales.

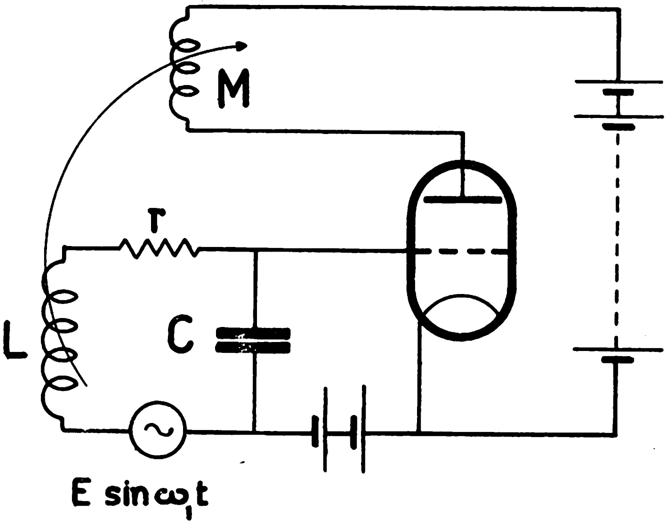

Fig. 1.

Soit un système à triode à un degré de liberté, tel que le représente la figure 1, où L, C, r constituent le circuit accordé et où la réaction est produite par l'induction mutuelle M.

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(1) Mémoire lu à l'Assemblée générale de l'U. R. S. I. à Washington, D. C., octobre 1927.

Soit en outre une F. E. M. E sin $\omega_1 t$, représentant l'action du signal incident sur le système oscillant. En négligeant le courant de grille et en désignant par i l'intensité du courant dans le circuit LCr, par i_a l'écart que présente l'intensité du courant de plaque de sa valeur permanente, et par v_g la différence de potentiel alternative de grille, nous avons

$$\mathrm{L}\frac{di}{dt} + ri + \frac{1}{\mathrm{C}}\int idt - \mathrm{M}\frac{di_a}{dt} = \mathrm{E}\sin\omega_1 t, \qquad (1)$$

$$\frac{1}{\mathrm{C}}\int idt = v_g. \qquad (2)$$

Dans le circuit de grille accordé, les variations de tension de plaque sont généralement petites devant les tensions de grille, de telle façon qu'en première approximation, la variation de l'intensité du courant de plaque i_a dépend seulement de la variation de la tension de grille, c'est-à-dire que

$$i_a = f(v_g). \qquad (3)$$

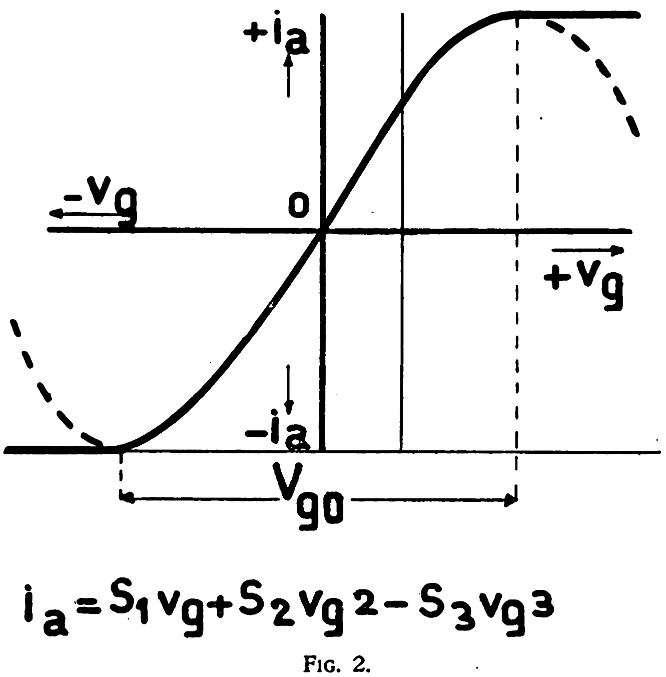

Fig. 2.

Dans un but de simplification, nous négligeons le courant de grille et nous supposons donc qu'il y ait une *tension* négative de grille. Notre système de coordonnées dans le plan de i_a, v_g est donc celui qui se trouve représenté sur la figure 2. Il diffère de la représentation habituelle en ce que l'origine des coordonnées est décalée vers le point correspondant au courant continu permanent autour duquel les oscillations se produisent.

A titre d'approximation nous remplaçons l'équation (3), qui représente la caractéristique incurvée de grille à tension de plaque constante de la triode par la cubique :

$$i_a = \mathrm{S}_1 v_g + \mathrm{S}_2 v_g^2 - \mathrm{S}_3 v_g^3 \qquad (3a)$$

où S_1 est la « conductance mutuelle » ordinaire pour les variations infinitésimales de la tension de grille. S_2 et S_3 sont déterminés ultérieurement par la forme de la caractéristique. Comme cette dernière s'incurve à la fois au sommet et à la base, nous écrivons, dans (3a), $-\ \mathrm{S}_3 v_g^3$ au lieu de $+\ \mathrm{S}_3 v_g^3$.

L'élimination de i et de i_a entre (1), (2) et (3) donne :

$$\left.\begin{aligned}\frac{d^2 v_g}{dt^2} + \left\{\left(\frac{r}{\mathrm{L}} - \frac{\mathrm{MS}_1}{\mathrm{LC}}\right) - \frac{2\mathrm{MS}_2}{\mathrm{LC}}v_g + \frac{3\mathrm{MS}_3}{\mathrm{LC}}v_g^2\right\}\frac{dv_g}{dt}\\ + \omega_0^2 v_g = \omega_0^2 \mathrm{E}\sin\omega_1 t.\end{aligned}\right\} (4)$$

où

$$\omega_0^2 = \frac{1}{\mathrm{LC}}.$$

Si nous posons, en outre,

$$\frac{r}{\mathrm{L}} - \frac{\mathrm{MS}_1}{\mathrm{LC}} = -\alpha,$$

$$\frac{\mathrm{MS}_2}{\mathrm{LC}} = \beta,$$

$$\frac{\mathrm{MS}_3}{\mathrm{LC}} = \gamma,$$

(4) devient

$$\ddot{v}_g + (-\alpha - 2\beta v_g + 3\gamma v_g^2)\dot{v}_g + \omega_0^2 v_g = \omega_0^2 \mathrm{E}\sin\omega_1 t, \qquad (4a)$$

c'est-à-dire une équation différentielle non linéaire et non homogène du second ordre dont les termes relatifs à la résistance ne sont pas linéaires.

L'approximation élémentaire habituelle d'une caractéristique linéaire donnerait $\beta = \gamma = 0$, et pour le cas de résonance (c'est-à-dire $\omega_0^2 = \omega_1^2$) et de la réaction critique (c'est-à-dire $\alpha = 0$) la différence de potentiel v_g qui se produit devient infinie. *Pour arriver à une théorie satisfaisante* *du comportement d'un système triode à réaction sous l'influence d'une F. E. M. imprimée, on doit résoudre préalablement un problème non linéaire.*

L'équation ($4a$) a été considérée en détail dans un mémoire antérieur (*) ; il résulte de ce travail, que la solution pour l'état stationnaire au voisinage de la résonance

$$(|\omega_0-\omega_1|\leqslant\omega_1)$$

peut s'écrire :

$$v_g = b\sin(\omega_1 t+\varphi),$$

où l'amplitude b de la variation résultante de la tension de grille est donnée par :

$$b^2\left\{4(\omega_0-\omega_1)^2+\left(\alpha-\frac{3}{4}\gamma b^2\right)^2\right\}=\omega_0^2\,E^2, \qquad (5)$$

équation du troisième degré en b^2. On voit en outre, par (5), que le terme asymétrique dans ($4a$) (avec β) (qui détermine la détection) n'a dans la première approximation envisagée ici, aucune influence sur le v_g résultant. *Donc, en première approximation, la détection n'a pas d'effet sur la différence de potentiel H. F. de grille qui se produit sous l'influence d'un « signal » incident.*

En outre, si l'on rapproche (5) de la relation

$$b^2\{4(\omega_0-\omega_1)^2+\alpha_0^2\}=\omega_0^2\,E^2$$

qui se rapporte au cas linéaire habituel, il s'ensuit dans le cas non linéaire, $(\alpha-\frac{3}{4}\gamma\, b^2)$ remplace α_0, c'est-à-dire que le système se comporte comme si, à la place de la résistance

$$r,$$

on avait substitué une résistance r' dont la valeur serait donnée par

$$r'=r-\frac{M}{C}\left(S_1-\frac{3}{4}b^2S_3\right), \qquad (6)$$

et qui dépendrait donc de l'amplitude b existant toujours dans le système (**).

Quand on pousse la réaction jusqu'à ce que

$$\frac{MS_1}{C}>r, \qquad (7)$$

on voit, grâce à (6), que la résistance différentielle de premier ordre du système devient négative et le système a donc la tendance à osciller spontanément. Cependant, comme on l'a montré dans le mémoire du Phil. Mag. cité ci-dessus, les oscillations forcées peuvent empêcher la naissance des oscillations libres, phénomène qui se manifeste par la présence d'une « zone de silence » s'étendant de part et d'autre de la résonance. Comme l'a montré le Prof^r^ Appleton (*), la largeur de la zone de silence est déterminée par l'amplitude du signal incident. Pour les signaux intenses cette largeur est donnée par

$$\frac{\omega_0-\omega_1}{\omega_0}=\pm E\,.\sqrt{\frac{3\gamma}{\alpha}}\,. \qquad (8)$$

Il est intéressant de chercher l'amplitude de grille résultante b

a, si le système est accordé exactement à la résonance, c'est-à-dire

$$\omega_0=\omega_1,$$

et **b**, si la condition d'amorçage des oscillations est réalisée, c'est-à-dire quand

$$S_1M=Cr. \qquad (9)$$

Dans ce cas, nous obtenons immédiatement, à partir de (5)

$$\frac{3}{4}\gamma b^3=\pm\,\omega_0 E, \qquad (10)$$

expression que nous allons maintenant envisager en détail.

En premier lieu, on déduit de (10) que *l'amplitude de grille résultante est proportionnelle à la racine cubique de la F. E. M. agissant sur le système.*

De plus, il est facile d'attribuer directement une valeur approximative d'après la caractéristique de la triode.

En se référant à ($3a$), ($4a$) et à la figure 2, et en prenant le cas symétrique pour lequel

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(*) Phil. Mag., **3**, 65, 1927.

(**) Cette propriété du système non linéaire a été démontrée pour la première fois en 1920, voir : Balth. Van der Pol, *Radio Review*, november, december 1920.

(*) E. V. Appleton, *Proc. Camb. Phil. Soc.*, **23**, 231, 1923.

$S_2 = 0$, il s'ensuit que l'on peut déterminer une bonne valeur approchée de S_3 à partir des valeurs maximum et minimum de la cubique (3*a*), pour $i_a = i_{a\,\max.}$ quand $v_g^2 = \frac{S_1}{3S_3}$.

En appelant « espace de grille » la variation de la tension de grille nécessaire à porter le courant de plaque de zéro à sa valeur de saturation, et en la désignant par V_{g0} (voir fig. 2), nous obtenons

$$V_{g0}^2 = \frac{4S_1}{3S_3},$$

d'où

$$\gamma = \frac{M}{LC}\,\frac{4S_1}{3V_{g0}^2}.$$

Mais, quand le système se trouve dans le cas de la réaction critique, la condition (9) est satisfaite, c'est-à-dire que

$$M = \frac{Cr}{S_1},$$

d'où

$$\gamma = \frac{r}{L}\cdot\frac{4}{3V_{g0}^2}.$$

Si nous désignons en outre par v_{g1} l'amplitude de la tension de la grille qu'on obtiendrait s'il n'y avait aucune réaction ($M = 0$) ou (ce qui revient au même) si le filament était moins chauffé, nous trouvons, grâce à (5),

$$v_{g1}^2 = \frac{\omega_0^2 L^2}{r^2}\cdot E^2,$$

d'où nous tirons, grâce à (10) :

$$b^3 = v_{g1}\cdot V_{g0}^2$$

ou

$$\boxed{b = \sqrt[3]{v_{g1}\cdot V_{g0}^2}} \qquad (12)$$

ce qui s'énonce :

L'amplitude de grille qu'on obtient dans le cas de la résonance et de la réaction critique, est égale à la racine cubique du produit de l'amplitude de grille qu'on obtiendrait sans aucune réaction, par le carré de « l'espace de grille » V_{g0}, tel qu'il est défini par la figure 2.

(12) peut de plus s'écrire :

$$\frac{b}{v_{g1}} = \left(\frac{V_{g0}}{v_{g1}}\right)^{2/3} \qquad (13)$$

ce qui signifie : *l'amplification obtenue par réaction est égale à la puissance deux tiers du rapport de « l'espace de grille »* V_{g0} *à l'amplitude obtenue avec une réaction nulle.* Il ressort immédiatement de (13) que le rendement obtenu par réaction avec les signaux faibles est beaucoup plus grand qu'avec les signaux intenses.

Quelques mesures ont fourni une vérification expérimentale très satisfaisante de la théorie exposée ci-dessus.

Les mesures furent effectuées à des fréquences relativement basses, en évitant donc des erreurs manifestes. Il ressort toutefois du théorème suivant que les résultats sont également applicables aux circuits de haute fréquence :

Si l'on réalise un modèle de système à haute fréquence composé d'inductances linéaires, de capacités linéaires, et de résistances non linéaires (triodes, par exemple) et si les valeurs de toutes les inductances (selfs et mutuelles) et capacités dans le modèle sont prises égales à n *fois ces valeurs dans le système à haute fréquence original, mais si les résistances (linéaires et non linéaires) dans le modèle sont prises égales dans le circuit original, les courants et potentiels qui se produisent dans ce modèle seront égaux en grandeur aux courants et potentiels dans le système original à haute fréquence, mais, si on les considère comme fonction du temps, ils varieront* n *fois plus lentement* (*).

Ainsi, les périodes propres du modèle seront égales à *n* fois celles du système original, et l'établissement et la décroissance des courants dans le modèle se produiront aussi *n* fois plus lentement.

Ce théorème simple découle immédiatement de la considération des dimensions des coefficients tels que

$$r, \quad \omega L, \quad \frac{1}{C\omega}, \quad M\omega,$$

qui interviennent dans les équations différentielles, car, par exemple,

$$L\omega = Ln\cdot\frac{\omega}{n}, \text{ etc.}$$

(*) Incidemment dans ce système modèle les capacités par dispersion sont réduites *n* fois en grandeur. C'est pourquoi, dans le but de chercher l'effet d'une capacité par dispersion déterminée dans le circuit original à haute fréquence, il suffit d'intercaler à sa place dans le circuit modèle une capacité égale à *n* fois la capacité par dispersion originale.

En effet, avec un système possédant une fréquence propre de 500 Hertz et dont la réaction était réglée à la valeur critique il fallait souvent une minute aux oscillations libres du système triode pour atteindre leur valeur finale, ce qui faisait qu'il n'était pas facile de savoir, avant d'appliquer la F. E. M. extérieure, si la réaction était exactement critique ou non. C'est pourquoi on choisit une voie intermédiaire et on construisit un modèle de récepteur possédant une fréquence propre de 15,000 Hertz.

On releva quatre séries de lectures (*) avec une triode étalon (filament de tungstène $V_f = 4.0$ volts, courants de saturation 11 m. A, tension de plaque variant entre 100 et 200 volts, tension négative de grille variant entre -4.5 et -7.5 volts).

La F. E. M. appliquée $E \sin \omega_1 t$ variait entre 2.10^{-6} et 10^{-2} volts, la tension alternative de grille résultante v_{g1} sans réaction variait entre 100.10^{-6} et 0.5 volts, et la tension alternative de grille résultante b avec réaction critique variait entre 0.2 et 4 volts environ. L'exposant S dans la formule

$$b = (V_{g0}^2 . v_{g1})^S$$

qui d'après la théorie ci-dessus, aurait pour valeur

$$S = 0.33$$

fut trouvé égal, d'après quatre séries de mesures, à

$$\begin{aligned} S = {} & 0.36 \\ & 0.36 \\ & 0.36 \\ & 0.32 \\ \hline \text{Moyenne : } S = {} & 0.35 \end{aligned}$$

alors que « l'espace de grille » V_{g0} calculé à partir des résultats de ces mesures, avait pour valeur

$$\begin{aligned} V_{g0} = {} & 26 \text{ volts} \\ & 25 \\ & 20 \\ & 15 \\ \hline \text{Moyenne : } V_{g0} = {} & 22 \text{ volts,} \end{aligned}$$

à rapprocher de la valeur

$$V_{g0} = 27 \text{ volts}$$

obtenue directement à partir de la caractéristique.

En particulier, la détermination expérimentale de l'exposant S est en bon accord avec la valeur théorique; à condition de n'envisager que les parties des caractéristiques auxquelles ne correspond aucune hystérésis dans l'accrochage. La représentation de la caractéristique i_a, v_g par une parabole du troisième degré, avec trois constants S_1, S_2 et S_3 seulement, ne peut évidemment pas fournir des valeurs très exactes pour V_{g0} obtenues, par exemple, à l'aide d'une variation de tension au plus égale à 0.35 volts.

Il est donc hors de doute que, pour toutes les nécessités de la pratique, la théorie exposée ci-dessus s'accorde bien avec l'expérience.

Nous donnons, enfin, quelques nombres calculés à partir de la formule (12), avec les données suivantes :

$$\frac{\omega L}{r} = 40,$$

$$V_{g0} = 27 \text{ Volts.}$$

Force électromotrice E agissant dans le circuit de grillage.	Tension alternative résultante de grille sans réaction (v_{g1}).	Tension alternative résultante de grille b, avec réaction critique.	Amplification qu'on peut obtenir avec une réaction critique (b/v_{g1}).
10^{-6} volts.	$0,04.10^{-3}$ volts.	0,31 volts.	7,700
10^{-5} »	$0,4 .10^{-3}$ »	0,66 »	1,600
10^{-4} »	$4,0 .10^{-3}$ »	1,4 »	360
10^{-3} »	0,04 »	3,1 »	77
10^{-2} »	0,4 »	6,6 »	16

Physical Laboratory
of Philips' Glowlampworks Ltd.
Eindhoven (Holland).

* * *

(*) Les expériences et étalonnages des amplificateurs nécessaires furent réalisées par MM. K. Posthumus et R. Veldhuyzen. On s'arrangea pour que le « récepteur » ne réagît pas sur l'émetteur.

L'étalon primaire de fréquence de la marine (1),

par Robert H. WORRAL et Raymond B. OWENS,
Naval Research Laboratory,
Washington (D. C., U. S. A.)

Résumé. — On décrit dans ce travail une source de fréquence constante qui est employée comme étalon primaire de fréquence par la Marine. Cette source de fréquence consiste en un oscillateur contrôlé par un cristal, de type spécial, qui fut construit au « United States Naval Research Laboratory », Washington, D. C. Cet oscillateur utilise un type de circuit riche en harmoniques, dont plus de deux cents peuvent servir d'étalons fixes de fréquence.

On décrit aussi en détail la méthode employée pour déterminer la fréquence fondamentale de cet étalon, en fonction du temps donné par le « Naval Observatory », avec une précision d'environ un cent-millième. L'étalon ultime de fréquence est donc le jour solaire moyen.

Historique. — Il y a quelques années, quand la Marine commençait à expérimenter les radiocommunications par ondes entretenues à haute fréquence, son étalon de fréquence était un ondemètre du type à absorption. Il consistait en un condensateur de précision et en un certain nombre de bobines construites pour présenter une self-induction constante et une faible résistance en haute fréquence. Un indicateur de résonance pouvait être mis, si on le désirait, en couplage lâche avec ce circuit. Cet ondemètre étalon était étalonné à l'aide du multivibrateur ou d'un autre oscillateur dont le débit était riche en harmoniques. Le multivibrateur était mis en résonance avec un diapason étalonné de fréquence 1,000. De la sorte, l'étalonnage de l'ondemètre pouvait être maintenu, par de fréquentes vérifications, à un degré de précision de un dixième de pour cent. Une précision de cet ordre suffisait pour l'émission par poste à étincelle et par ondes entretenues à fréquence moyenne.

Un des premiers résultats de l'avènement de la transmission à très haute fréquence fut d'indiquer l'insuffisance d'un étalon de fréquence qui pût présenter une erreur d'étalonnage de 0.1 % dans chaque sens. Car, à une fréquence de 10,000 kilocycles, l'erreur possible eut été de 10 kilocycles par excès ou par défaut. Ainsi, à cette fréquence, un poste de transmission eût immobilisé une gamme de 20 kilocycles, ce qui eût augmenté l'interférence entre les stations, ou diminué fortement le nombre de voies de communication, en exigeant une séparation plus étendues en kilocycles entre les fréquences assignées. On conçoit naturellement qu'un ondemètre d'usage courant calibré d'après cet étalon eût pu comporter un pourcentage d'erreurs plus grand que l'étalon lui-même.

Il se présentait ainsi, devant le « Naval Research Laboratory » un triple problème :

1° Construire pour le service de la Marine un étalon de fréquence aussi stable que possible;

2° Fournir un moyen d'obtenir directement à partir de cet étalon un grand nombre de fréquences fixes pour des étalonnements;

3° Trouver une méthode pour déterminer la fréquence de cet étalon en fonction du temps de l'observatoire et avec une précision meilleure que 0.01 % se rapprochant même autant que possible de 0.001 %.

Oscillateurs contrôlés par un cristal. — L'application des propriétés piézo-électriques des cristaux de quartz au contrôle de la fréquence des circuits oscillants à lampes, insensibles aux petites variations dans la capacité ou la self-induction, suggéra l'idée d'employer un oscillateur contrôlé par un cristal à la solution de notre premier problème. Après avoir expérimenté divers types de circuits et apporté un certain nombre de perfectionnements aux supports des cristaux, on construisit un oscillateur contrôlé par un cristal avec lequel un contrôle automatique de la température donna, comme on put en juger, un débit d'une constance pratiquement absolue.

Les essais ont montré que l'effet produit sur la fréquence de l'oscillateur étalon par une variation de 5 % dans la tension de plaque, est inférieur à 0.001 %. Une variation de 10 % dans la tension du filament ne produit aucun changement appréciable dans la fréquence. Le remplacement d'une lampe par une autre du même type produit une variation de fréquence allant de zéro à un maximum d'environ 0.002 %. Pour tenir compte du manque d'uniformité dans les constantes des lampes, plusieurs lampes qui donnent la même fréquence que la lampe oscillatrice employée dans l'étalonnage, sont gardées en réserve, ce qui permet d'éliminer pratiquement l'erreur due aux lampes. Le coefficient de température du cristal étalon dans son support est inférieur à un cent-millième par degré centigrade.

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(1) Lu par le Dr. L.-P. Wheeler.

Types de circuits. — La figure 1 représente un type de circuit employé pour les travaux préliminaires. Le débit de ce circuit de plaque accordé (1) qui utilise une self-induction petite et une capacité relativement grande, est maximum pour la fréquence fondamentale et diminue rapidement pour les harmoniques. En d'autres termes, c'est un circuit du type transmetteur, et l'on ne peut utiliser que quelques harmoniques pour les fins d'étalonnage. On modifia donc ce circuit, afin de réduire l'intensité de la fréquence fondamentale et d'augmenter celle des harmoniques, de telle sorte que chaque multiple de la fréquence du cristal pût être employé comme étalon de fréquence. Cette modification consista à remplacer le circuit de plaque accordé par une self-induction très grande ayant elle-même une petite capacité propre et sans condensateur (voir fig. 2). Cette bobine non accordée doit avoir une fréquence propre supérieure à la fréquence fondamentale du cristal, c'est-à-dire qu'il doit y avoir une réactance inductive à la fréquence du cristal pour donner au potentiel de grille une réaction dans le rapport de phase convenable pour entretenir des oscillations. Afin de mieux faire ressortir la différence dans l'intensité des harmoniques entre ces deux types de circuits de plaque, on peut dire que la valeur du courant de haute fréquence dans le circuit de plaque d'un oscillateur sur une fréquence d'harmonique donnée, dépend dans une grande mesure du rapport de l'impédance du circuit extérieur pour cette fréquence, à l'impédance intérieure de plaque de la lampe. Quand ces impédances sont à peu près égales, pour une fréquence donnée, on obtient l'énergie maxima à cette fréquence. Si l'impédance du circuit extérieur est petite par rapport à l'impédance de la lampe, à une fréquence donnée, l'énergie rayonnée à cette fréquence sera en conséquence petite.

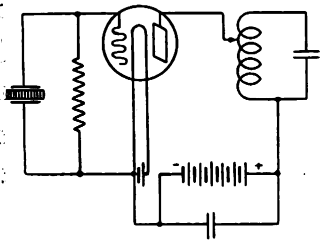

Fig. 1.

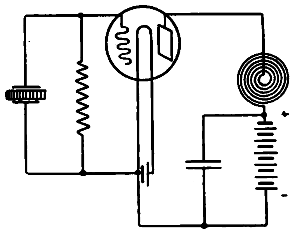

Fig. 2.

Dès lors le circuit accordé, avec un faible rapport L/C, donne une impédance extérieure de l'ordre de celle de la plaque de la lampe, ou plus grande, sur une gamme de fréquences étroite (voir fig. 3*a*). Par exemple, l'impédance extérieure est relativement très petite à une fréquence égale à cinq fois la fréquence fondamentale du cristal. L'énergie de cet harmonique serait donc très faible devant l'énergie rayonnée sur la fréquence fondamentale.

D'autre part, une bobine dont le rapport L/C est grand donne une courbe d'impédance très large, indiquant que l'intensité de la fréquence fondamentale et d'un grand nombre d'harmoniques successifs sera du même ordre, comme l'indique la figure 3*b*.

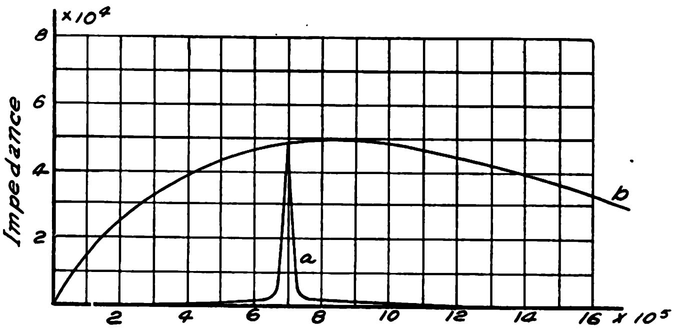

Fig. 3.

Cet oscillateur peut être considéré comme un émetteur à très faible puissance, contrôlé par un cristal, capable de rayonner sur deux cents fréquences environ dont chacune est un multiple de la fréquence fondamentale du cristal.

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(1) Construit par le Dr. J.-M. Miller.

Pour utiliser tous ces harmoniques, comme dans l'étalonnage des fréquencemètres hétérodynes, il est nécessaire de coupler cet oscillateur, ainsi que tout oscillateur qui doit être étalonné d'après lui, à un détecteur et un amplificateur. L'oscillateur à cristal, ainsi que le détecteur et l'amplificateur sont montés en un seul appareil qui, complété par le système de contrôle automatique de la température, constitue le «*Crystal controlled standard oscillator* » (Oscillateur étalon contrôlé par un cristal) (voir fig. 4). Le circuit du détecteur est apériodique et la bobine est destinée à fonctionner à toutes les fréquences sans aucun réglage. Dans la pratique, le couplage a lieu entre le circuit de l'oscillateur à cristal et la bobine du détecteur, et un fil de couplage partant du détecteur est placé suffisamment près de l'oscillateur à étalonner pour que le couplage soit réalisé. Quand le fréquencemètre hétérodyne (ou tout autre type d'oscillateur à variation continue) est accordé pour donner une fréquence de battements acoustiques avec un harmonique de l'oscillateur à cristal, la note du battement est redressée par le détecteur et amplifiée par un ou deux étages d'amplification.

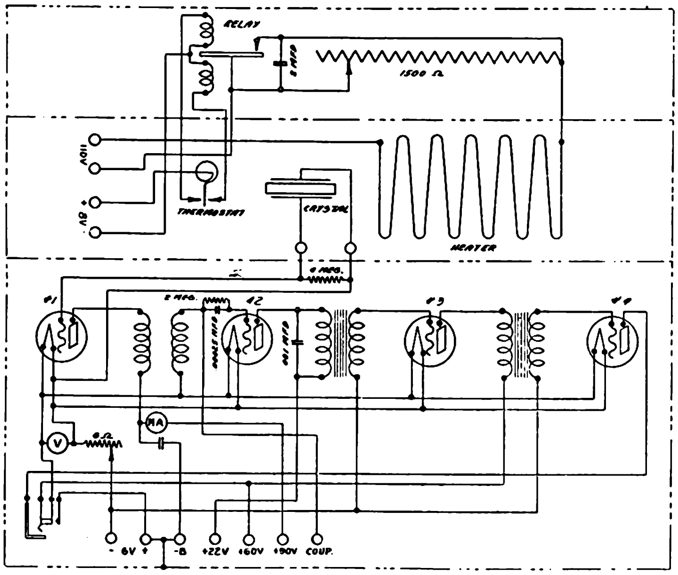

Fig. 4.

La diminution dans l'intensité de la note de battement entre les harmoniques successifs de la fréquence fondamentale du cristal et un autre oscillateur, est trop faible pour pouvoir être notée. Le signal produit par le centième harmonique est du même ordre d'audibilité que celui produit par le cinquantième, et le deux centième harmonique donne une note d'intensité aisément appréciable. Cet appareil est portatif comme on peut le voir sur les figures 5 et 6. Au « Naval Research Laboratory » la fréquence du cristal étalon est 25 kilocycles. A l'aide de ce seul cristal et des circuits associés, comme on vient de le décrire, on peut obtenir des fréquences-étalons tous les 25 kilocycles jusqu'à 5,000 kilocycles. Pour une gamme de plus haute fréquence, on dispose d'un autre étalon dont le cristal possède une fréquence de 250 kilocycles. Ces deux étalons de fréquence permettent de recouvrir toute l'échelle des fréquences de la radiotélégraphie.

ETALONNAGE DE L'OSCILLATEUR ÉTALON.

Description générale de la méthode utilisée. — A présent qu'une source stable de fréquences existe, capable de fournir environ deux cents repères pour les étalonnages, il restait à trouver un procédé de détermination de la fréquence de cet étalon qui donnat une approximation aussi voisine que possible de 0.001 %.

La méthode qui a été finalement adoptée convient particulièrement bien à l'étalonnage des sources à fréquence basse, comme l'étalon de 25 kilocycles, mais elle n'est pas limitée à une telle fréquence. En résumé, la méthode employée utilise un générateur entraîné par un moteur, dont on fait varier la fréquence jusqu'à ce qu'un harmonique du courant du générateur donne avec la fréquence fondamentale de l'oscillateur à cristal un battement nul. Le générateur est ensuite entraîné exactement à cette fréquence pendant quelques minutes, durant lesquelles on procède à un enregistrement : un marteau imprime toutes les secondes un point sur une feuille de papier, placée sur un tambour de chronographe; ce marteau est commandé par un courant électrique déclanché par un chronomètre étalonné. La fréquence du générateur est déterminée très exactement par l'inclinaison de la ligne de points marqués sur le tambour du chronographe. Cette fréquence, multipliée par le chiffre correspondant à l'harmonique du générateur qui était au battement nul avec l'oscillateur à cristal, donne la fréquence de l'oscillateur lui-même.

Pour montrer comment on applique la méthode dans la pratique, nous donnons ci-dessous une description détaillée des appareils et des procédés.

Appareils utilisés dans l'étalonnage de l'oscillateur étalon contrôlé par un cristal.

1) Un moteur générateur à 500 périodes.
2) Un chronomètre très exact.
3) Un chronographe.
4) Un pont de Maxwell.
5) Un circuit destiné à supprimer la fréquence fondamentale 500 du générateur et à accentuer les harmoniques de cette fréquence.
6) Un amplificateur spécial.

1) Le moteur générateur consiste en un moteur à courant continu de cinq H. P. relié par un lourd volant à un alternateur à 500 périodes et alimenté par une batterie de 110 volts à forte capacité (voir fig. 7).

2) Le chronomètre est un grand chronomètre étalon de la Marine réglé et vérifié périodiquement au « Naval Observatory ». Le chronomètre est contrôlé tous les jours au laboratoire à l'aide des signaux horaires étalons. L'écart déterminé à l'Observatoire était de une seconde et demie par jour. Nos observations recouvrant plusieurs années montrent une légère variation saisonnière de cet écart, de l'ordre de une seconde par jour en été et une seconde et demie en hiver. Une table de corrections a été dressée, donnant l'écart à tout moment de l'année. Mais lorsqu'on doit faire des étalonnages du cristal, l'écart est déterminé grâce à la vérification quotidienne par les signaux horaires.

3) Le chronographe se compose des parties suivantes : le chronomètre, un relais, un marteau et un tambour. Le tambour, figure 8, est un cylindre creux en cuivre d'un diamètre de 7″ et d'une longueur de 16″, commandé avec précision et engrené au générateur à l'aide d'engrenages réducteurs, donnant une révolution du tambour pour 150 révolutions de l'arbre du générateur.

Une feuille de papier est placée sur ce cylindre, et le marteau mentionné plus haut y produit un enregistrement en se déplaçant le long du tambour au moyen d'une vis sans fin. Le marteau donne un point d'enregistrement par seconde (excepté la 59^e seconde de chaque minute) et est commandé par le chronomètre.

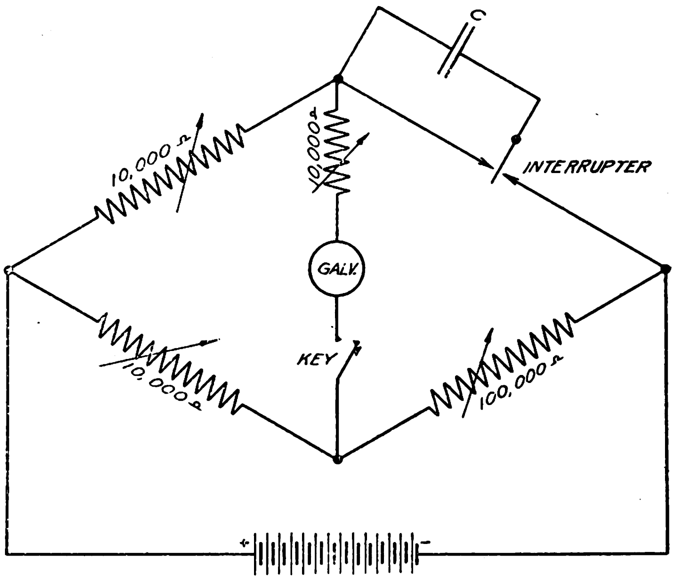

Fig. 9. — Pont de Maxwell.

Une révolution de l'arbre du générateur correspond à 20 alternances de la tension, comme on peut l'observer en comptant les pôles. Quand le tambour, actionné par le train d'engrenages réducteurs, fait exactement un tour en six secondes, comme cela sera indiqué par six rangées de points rigoureusement parallèles à l'axe du tambour sur la feuille enregistreuse, la fréquence du générateur est de $\frac{150 \times 20}{6}$ soit 500 périodes par seconde.

Si les rangées de points donnent une inclinaison ascendante, la fréquence du générateur est inférieure à 500 périodes; si l'inclinaison est descendante, elle est supérieure. La fréquence exacte du générateur pendant une révolution est déterminée à partir de cet enregistrement au chronographe, comme on l'expliquera en détail plus loin.

4) Le pont de Maxwell, dont la figure 9 représente un schéma, est employé pendant le

calibrage du cristal étalon comme auxiliaire pour maintenir la vitesse du moteur générateur, constante à la valeur désirée.

Le condensateur représenté sur l'une des branches, est chargé et déchargé par la batterie qui est en parallèle avec le pont, à l'aide d'un interrupteur tournant commandé par l'arbre du générateur. Les segments du commutateur de l'interrupteur chargent et déchargent le condensateur au moyen de trois balais.

Quand le pont est équilibré pour une vitesse donnée du générateur, la moindre variation de cette vitesse provoque une déviation du spot lumineux réfléchi par le miroir du galvanomètre sur une échelle en verre dépoli. Le sens du déplacement du spot lumineux indique si le générateur tourne trop lentement ou trop vite, ce qui permet à l'opérateur de varier la pression sur le volant pour ramener la vitesse du générateur à la condition de battement nul.

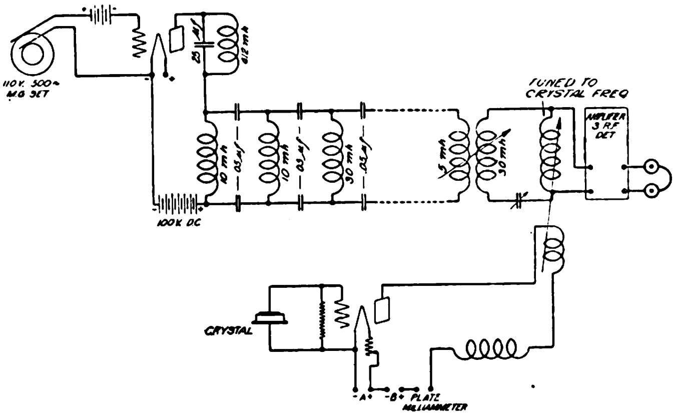

Fig. 10. — Circuit d'étalonnement du cristal.

Cet emploi du générateur, du chronographe et du pont de Maxwell, procède de l'appareil utilisé au « Bureau of Standards » (1) pour les mesures exactes de petites capacités.

5) La méthode de production des harmoniques d'un courant de 500 périodes environ consiste à imprimer cette fréquence au circuit de grille d'une lampe, dont le circuit de plaque est riche en harmoniques désirés. Comme on peut s'en rendre compte par la figure 10, un circuit de résonance accordé, dans le circuit de plaque, absorbe partiellement la fréquence fondamentale 500 qui est, naturellement très intense. Le reste du circuit consiste en un filtre qui ne laisse passer que les harmoniques de haute fréquence du générateur. Ce circuit filtre se termine par une bobine de couplage, couplée à un circuit accordé à la fréquence du cristal. Ce circuit accordé est également couplé à un amplificateur au moyen duquel les battements entre les harmoniques du cristal et du générateur sont amplifiés et transmis aux téléphones.

6) L'amplificateur comprend trois étages de haute fréquence et un détecteur.

Conduite d'un essai. — La fréquence du cristal étalon est déterminée d'abord approximativement à l'aide d'un ondemètre étalon. Celui-ci est exact à 0.1 % près.

On place une feuille de papier sur le tambour et le générateur est mis en marche. Après que le générateur s'est mis en régime pendant trente minutes environ, les téléphones de l'amplificateur représentés sur la figure 10 sont introduits dans la salle du générateur. L'opérateur, une main sur le volant du moteur générateur fait lentement varier la vitesse du générateur de 500 périodes jusqu'à ce qu'un multiple de cette fréquence ou un harmonique s'approche de la fréquence du cristal et que des battements soient entendus dans les téléphones. L'opérateur règle alors la vitesse jusqu'à ce que l'on ne perçoive plus que de légers battements. Avec l'aide de l'autre opérateur, le pont de Maxwell est équilibré, ce qui maintient le spot immobile sur l'échelle. Cela se fait pendant que le premier opérateur corrige les légères variations dans la vitesse du générateur en modifiant la pression sur le volant.

Le seul rôle du pont est d'indiquer à l'opérateur par le sens de la déviation du spot lumineux, si les battements rapides dans les téléphones représentent une augmentation ou une diminution dans la vitesse du générateur, de telle sorte qu'il puisse rapidement augmenter ou diminuer la pression sur le volant pour ramener la vitesse du générateur à sa juste valeur indiquée par de très lents battements dans les téléphones. La hauteur du son dans les téléphones est celle de la fréquence fondamentale du générateur, fait qui peut s'expliquer comme suit : quand un harmonique de la fréquence du générateur est au battement nul avec la fréquence du cristal, les harmoniques de rang immédiatement supérieur et inférieur donnent une note de battement avec la fréquence du cristal. Si l'harmonique de 25,000 périodes est à l'unisson, avec la fréquence du cristal, ceux de 24,500 et de 25,000 donnent une note à 500 périodes avec le cristal. Les pulsations ou battements dans cette note indiquent combien l'harmonique à 25,000 périodes diffère de la fréquence du cristal. Si on entend quelques battements par excès, on réduit la vitesse du générateur juste assez pour qu'elle produise le même nombre de battements par défauts par rapport à la position de battement nul. Un opérateur habile peut donc compenser de légères variations dans la fréquence de battement pendant un essai.

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(1) *Bull. Bureau of Standards*, vol. III, # 4.

L'opérateur règle alors la vitesse du générateur pour obtenir une fréquence de battement aussi lente que possible pendant quelques minutes avant de commencer à enregistrer l'essai. Quand il observe que l'appareil est suffisamment stable, il ferme le circuit du chronomètre et le marteau commence à produire un enregistrement sur le papier monté sur le tambour. Le temps exigé pour faire un essai varie de huit à quinze minutes.

Après l'essai, le second opérateur peut faire une contre épreuve avec le même harmonique, et ensuite les deux opérateurs font des essais sur un autre harmonique. On n'observe dans les résultats aucune erreur imputable à un élément humain.

Production de la fréquence du générateur. — Avant de déplacer le papier enregistreur du tambour, on y trace une ligne parallèle à l'axe de celui-ci. L'enregistrement complet contient six rangées parallèles de points puisque le tambour fait une révolution complète en six secondes environ, suivant la fréquence du cristal. Le marteau forme un point sur le papier toutes les secondes, pendant que le chariot sur lequel repose le marteau avance approximativement de 1 millimètre par révolution du tambour. Une partie de l'enregistrement complet est représentée sur la figure 11.

La méthode employée pour évaluer la fréquence du générateur est basée sur le fait que, étant donné le nombre de pôles du générateur et le rapport de réduction de l'engrenage entre le générateur et le tambour du chronographe, mentionné plus haut, une révolution du tambour en six secondes exactement correspond à une fréquence du générateur d'exactement 500 périodes. Cela signifie que, si les six rangées parallèles de points imprimés sur le tambour par le chronomètre sont parallèles à l'axe du tambour, le générateur fonctionne à 500 périodes.

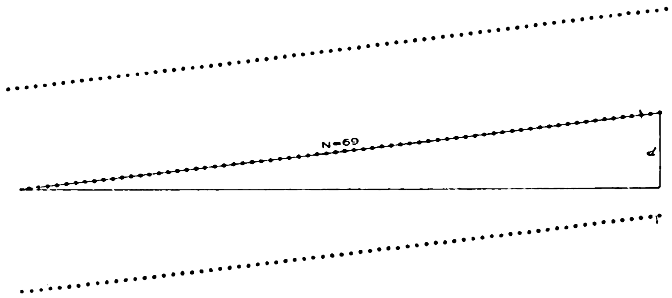

FIG 11

Si l'inclinaison de ces rangées de points est ascendante, le tambour n'a pas fait une révolution complète en six secondes et, par suite, la vitesse du générateur est inférieure à 500 périodes. Une inclinaison descendante indique plus d'une révolution du tambour dans l'intervalle de six secondes, ou une vitesse du générateur supérieure à 500 périodes.

Supposons par exemple que les rangées de points soient parallèles à l'axe du tambour. Alors, en 100 rotations, un point sur la surface du tambour aurait parcouru 100 fois la circonférence du tambour, soit 100 × 51.9 cm. Cela correspond à une vitesse du générateur de 500 périodes. On peut donc déterminer la fréquence du générateur pour toute inclinaison par une simple proportion :

$$CN : CN \pm d = 500 : X$$

où C = la circonférence du tambour.
N = le nombre de révolutions du tambour.
d = la différence de hauteur le long de la perpendiculaire entre le premier et le dernier point d'une rangée.
X = la fréquence du générateur.

La grandeur d ci-dessus est la distance dont varie le parcours total, ce qui est un nombre pair de révolutions du tambour. Si la pente est ascendante, d est négatif ; si elle est descendante, d est positif.

Exemple :

$C = 51.9$

$N = 100$

$d = 0.3$ cm. Pente ascendante. Donc d négatif.

Fréquence du générateur = 499,971 périodes par seconde.

La mesure de la fréquence du cristal par l'ondemètre étalon, indique une valeur approximative de 25 kilocycles. C'est donc nécessairement le cinquantième multiple de la fréquence fondamentale du générateur qui était maintenu à la fréquence fondamentale du cristal pendant l'essai.

La fréquence de l'oscillateur à cristal est donc égale à cinquante fois la fréquence du générateur, soit 24,998.55 périodes. Si la vitesse du générateur était augmentée suffisamment, on trouverait un point aux environs de 510 périodes où la condition de battement nul peut être située à nouveau à l'aide de téléphones. Il est clair que c'est là le quarante-neuvième harmonique du générateur réglé à la fréquence du cristal ($510 \times 49 = 25{,}000$ approximativement).

Précision de la méthode. — La précision d'un étalonnage dépend surtout de la constance à laquelle peut être maintenue la fréquence du générateur. C'est pour cette raison que le moteur générateur est alimenté par un groupe de batteries d'accumulateurs de forte capacité maintenus en bon état et complètement chargés. Il faut, avant de commencer l'essai, que le régime du groupe soit complètement établi. La batterie du pont de Maxwell est également vérifiée soigneusement au point de vue de la constance de la tension, et les balais et segments de contact dans le circuit du pont sont soigneusement entretenus et bien ajustés. Comme on l'a indiqué plus haut, de légères variations dans la fréquence du générateur peuvent être compensées par l'opérateur, mais si l'essai est notablement défectueux il est enregistré tel quel ou écarté.

L'erreur qui provient de la mesure de la distance d est réduite au minimum en la déterminant pour les six lignes parallèles d'un enregistrement, et en utilisant la moyenne comme valeur exacte pour d. L'erreur résultante ne dépasse pas un deux cent millième. Dans ces conditions, l'erreur totale pour un bon essai est couverte par 0.001 %. Ce degré de précision n'est possible que lorsque le cristal n'est pas troublé. L'exactitude qu'on peut exiger pour l'oscillateur étalon à cristal utilisé comme étalon portatif est de 0.002 %. L'inexactitude due à l'erreur probable de la mesure de la circonférence du tambour est imperceptible.

Pour donner une idée de la correspondance entre divers étalonnages, nous donnons un tableau de huit essais consécutifs sur un cristal étalon. Il faut noter que l'erreur probable dans la valeur moyenne est le quotient de trois par deux millions et demi.

Tableau d'étalonnages.

Date.	Température en degrés C.	Fréquence en périodes.
15-8-1927	38.45	24,998.60
15-8-1927	38.45	24,998.75
16-8-1927	38.45	24,998.71
16-8-1927	38.45	24,998.73
29-8-1927	38.55	24,998.99
29-8-1927	38.55	24,998.82
9-8-1927	38.50	24,998.71
9-8-1927	38.50	24,998.63
	Valeur moyenne =	24,998.74

Emplois de l'oscillateur étalon contrôlé par un cristal. — Cet oscillateur étalon est un appareil portatif, dont les dimensions sont de $8 \times 14 \times 22\ 1/2$ pouces. Il est facile à manier, car il n'y a pas d'éléments variables ni de dispositif d'accord. Il n'exige qu'une tension de plaque de 90 volts, et le réchauffeur peut être branché sur le secteur de 110 volts. On peut l'employer pour deux cents fréquences fixes ou plus, régulièrement espacées en kilocycles et exactes à 0.001 % près. L'invariabilité de l'étalonnage du cristal ne dépend que de l'invariabilité du cristal lui-même.

On emploie cet oscillateur au « Naval Research Laboratory » pour étalonner tous les cristaux de quartz qu'on y fabrique. Au moyen d'un fréquencemètre hétérodyne de précision, ayant une caractéristique de fréquences linéaire, on peut étalonner un cristal d'une fréquence quelconque, à partir de l'oscillateur étalon, à 0.002 %.

En outre, tous les fréquencemètres hétérodynes destinés au « Naval Service » sont étalonnés directement d'après les harmoniques d'un oscillateur étalon. On peut faire les lectures aussi vite que le permet l'accord du fréquencemètre hétérodyne sur les harmoniques successifs de l'oscillateur à cristal. Pour un travail de très haute précision, on fait les lectures sur la note de battement de 1,000 périodes (déterminée par un diapason) de part et d'autre du point de battement nul.

L'étalonnage d'un fréquencemètre hétérodyne peut être rapidement vérifié, dans le but de déterminer l'erreur à un instant quelconque, en faisant les lectures pour quelques harmoniques du cristal. A cette fin, un type spécial d'oscillateur à cristal sans réglage de température a été fourni à la Flotte sous la dénomination de « Cristal Controlled Calibrator Navy type » # S. E. 2907.

L'oscillateur étalon peut aussi servir à la comparaison des étalons de fréquences. On a déjà fait une pareille comparaison entre l'oscillateur étalon courôlé par un cristal, de la Marine et le diapason étalon des « Bell Telephone Laboratories » pour déterminer la constance relative et la concordance dans l'étalonnage des deux étalons. Pour cet essai, on transporta l'oscillateur de la Marine à New-York City. Un certain nombre d'enregistrements par oscillographe de la fréquence de battement entre les harmoniques des deux étalons, pris à intervalle de trois jours, ont montré qu'il n'y avait pas de variation appréciable dans la constance relative des deux sources. La détermination de fréquence par les deux étalons différait de un quatre-vingt millième ou 0.0012 %. Cette concordance peut être considérée comme excellente quand on se rappelle que l'étalon de la Marine avait été transporté de Washington à New-York pour l'essai et que l'on employait des méthodes entièrement différentes pour étalonner les deux étalons.

Depuis que fut faite cette comparaison avec l'étalon des « Bell Telephone Laboratories », ce Laboratoire a construit quatre oscillateurs contrôlés par un cristal, semblables à celui décrit ici, réalisant ainsi une partie du programme élaboré en vue de l'établissement d'un étalon national secondaire de fréquence, dont la création fut décidée à une conférence à Washington, en février 1927. Des représentants des divers départements gouvernementaux intéressés, et un certain nombre de sociétés de constructions radioélectriques assistaient à cette conférence.

Il faut espérer qu'à la suite de la détermination de la fréquence de ces oscillateurs dans plusieurs autres laboratoires, et de l'obtention d'un résultat moyen, ces oscillateurs pourront servir de précieux étalons secondaires de fréquence pour le gouvernement et de l'industrie radioélectrique du pays.

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Les observations radiogoniométriques sur les atmosphériques au Japon,

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I. *Introduction.* — Depuis février 1927, nous avons effectué des observations quotidiennes sur la direction des atmosphériques qui troublent la réception des signaux de grande longueur d'onde, dans notre Laboratoire, à Isohama Town qui est situé à 100 kilomètres environ au nord-est de Tokyo, en face de l'océan Pacifique. Quoique la durée des observations ait été trop courte pour pouvoir en tirer des conclusions définitives, nous saisissons l'occasion de ce Congrès pour présenter les résultats acquis.

II. *Appareils et méthode d'observation.* — On a utilisé un radiogoniomètre Telefunken du type E-276-b. Il consiste en un récepteur hétérodyne qui comporte un amplificateur haute fréquence à trois étages, un détecteur et un amplificateur basse fréquence à deux étages, combinés à des cadres circulaires interchangeables de $0^{m}85$ de diamètre pouvant tourner autour d'un axe vertical, de manière à pouvoir opérer dans une gamme étendue de longueur d'onde, et enfin une petite antenne verticale qui peut rendre le récepteur unidirectionnel. Comme la longueur d'onde maximum reçue avec l'appareil était de 7,200 mètres, nous avons préparé un autre cadre pour couvrir les longueurs d'onde plus longues, nécessitées par les observations actuelles, et l'antenne verticale fut remplacée par une autre plus longue. L'antenne verticale était coupée au cadre par sa bobine de charge, la condition de résonance étant réalisée à l'aide d'un condensateur variable en parallèle avec la bobine, Le cadre possédait aussi un condensateur d'accord. Avec ce montage, on pouvait aisément obtenir des caractéristiques de réception en cardioïde, la phase de la force électromotrice induite étant obtenue en agissant sur l'accord de chaque circuit et sur l'antenne par le degré de couplage de la bobine.

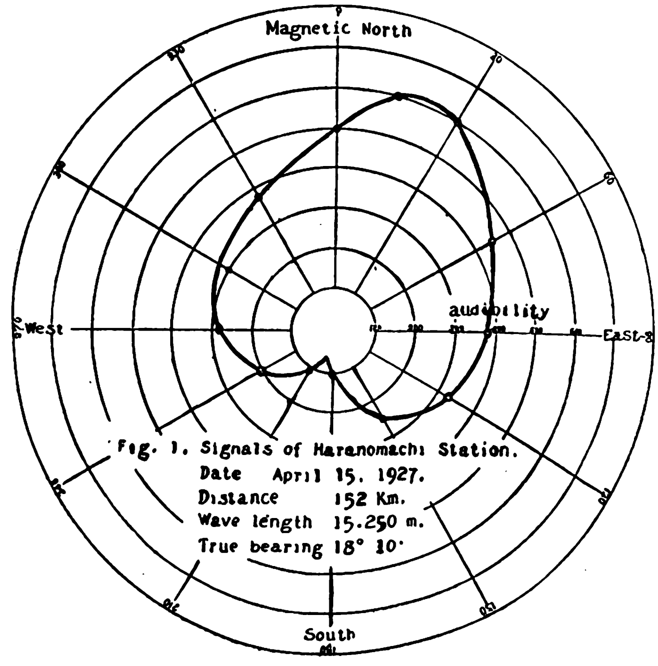


Fig. 1. Signals of Haranomachi Station.
Date April 15. 1927.
Distance 152 Km.
Wave length 15.250 m.
True bearing 18° 10'

Les atmosphériques, surtout ceux qui ont la nature de claquements (clicks) varient généralement en intensité et en direction d'un moment à l'autre, aussi n'est-il pas aisé de tracer la courbe de répartition en coordonnées polaires, pour des claquements (clicks) ou même pour des grondements (grinders). Afin de simplifier le travail et d'augmenter la précision des observations actuelles, on a adopté une méthode d'observations en un point dans laquelle on doit d'abord déterminer une direction en tournant le cadre et en découvrant une position qui corresponde à la lecture minima; on détermine après le sens en ajoutant l'antenne verticale au cadre, une combinaison unidirectionnelle étant sujette à des erreurs plus importantes qu'un simple cadre en radiogoniométrie.

III. *Courbes polaires unidirectionnelles.* — La figure 1 est un exemple de courbe polaire unidirectionnelle pour la réception des signaux, et la figure 2 pour les « grinders ». Ainsi que le montre la figure, aucun diagramme polaire pour les atmosphériques n'est, comme on sait, en forme de cardioïde, ce qui constitue un phénomène intéressant pour des études ultérieures.

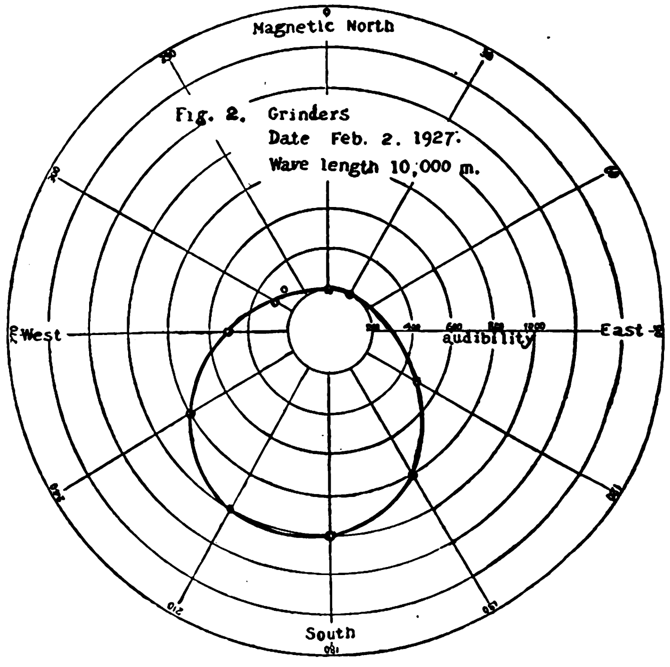


Fig. 2. Grinders
Date Feb. 2. 1927.
Wave length 10,000 m.

IV. *Précision des observations radiogoniométriques et correction des appareils.* — Pour les observations de signaux intenses, on a trouvé que l'appareil possédait une précision de 1 degré. Mais pour les observations effectuées sur les parasites, la largeur de la région de lecture minima était de 2 à 10 degrés et atteignait même parfois 15 degrés, la largeur de cette région dependant du moment de la journée et de la saison, ce qui pourrait indiquer que le siège des atmosphériques recouvre une aire étendue ou se déplace d'un endroit à l'autre. Comme les claquements « clicks » qui se produisent par intermittence à de courts intervalles de temps et qui varient beaucoup plus dans les directions observées, ont été bien plus malaisés à observer, on peut présumer que la précision des mesures qui s'y rapportent a été bien inférieure à celle des « grinders ».

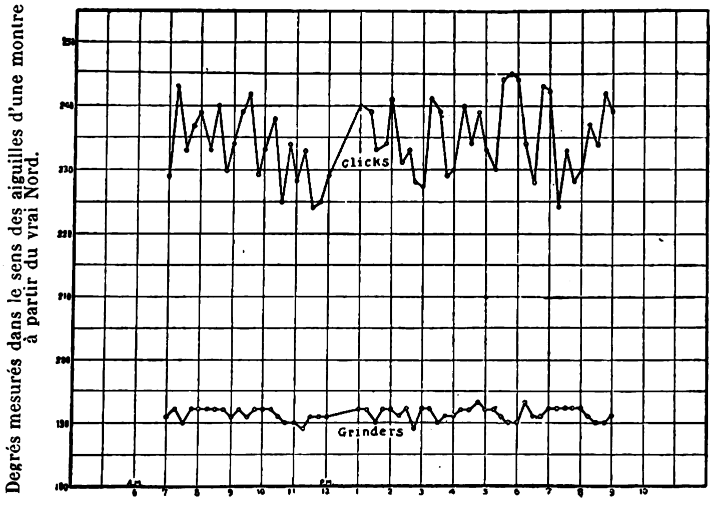
En plus de ce fait, il existait, autour des appareils de mesure, plusieurs conducteurs fixes qui pouvaient provoquer certaines erreurs dans les observations goniométriques. Pour corriger grossièrement ces effets, les azimuts apparents de plusieurs stations situées à des endroits connus furent déterminés par les mesures et comparés à leur azimut réel. On a trouvé ainsi que les écarts maxima étaient seulement de 1 à 2 degrés, et variaient avec la direction. Dans les nombres de la table I ci-dessous, on n'a pour cela introduit aucune correction.

En outre, on a fait toutes les observations en comptant les emplacements dans le sens des aiguilles d'une montre en degrés, à partir du Nord magnétique. Pour déterminer les emplacements analogues mesurés à partir du Nord vrai, on devait tenir compte de la déclinaison magnétique. Comme on peut estimer la déclinaison à 5°00' W. en moyenne, au poste récepteur, on a corrigé les nombres de la table I ci-dessous en retranchant 5 degrés aux positions observées.

V. *Résultats des observations.* — 1) Les observations furent commencées le 5 février. Jusqu'au 7 mars, seules les observations relatives aux grondements ont été faites sur diverses longueurs d'onde comprises entre 10,000 et 21,000 mètres, à peu près dix fois par jour de 9 à 16 heures. Aucune variation appréciable due aux longueurs d'onde, n'a cependant été notée, leur amplitude étant comparable à celle de la variation diurne observée sur une longueur d'onde particulière.

2) Ensuite on s'est borné aux observations de deux longueurs d'onde particulières, savoir 10,000 et 20,000 mètres. Toutefois, la comparaison des lectures faites sur ces deux longueurs d'onde montre entre elles une identité en ce qui concerne la direction.

3) Après le 16 mars, la durée des observations a été prolongée de 7 à 21 heures, les observations étant faites presque toutes les trente minutes. La variation était, en général, moindre le jour que la nuit, bien que cela ne fût pas aussi remarquable que dans le cas des observations de signaux. Cela semble indiquer que le siège des atmosphériques n'est pas fixe.



Temps central étalon du Japon.

FIG. 3. — Variation diurne dans les directions des « grinders » et « clicks », le 26 mai 1927. Longueur d'onde 10,000 mètres.

4) Depuis avril, on a observé les « clicks » en même temps que les « grinders ». On a trouvé que les directions des « clicks » sont très différentes de celles des « grinders » et que les variations diurnes dans les directions des « clicks » sont beaucoup plus grandes que dans celles des « grinders ». La figure 3 donne un exemple des variations diurnes dans les directions des deux types. Il semble en résulter que le siège des « clicks » est différent de celui des « grinders ».

5) Des observations de « grinders » de février à juillet, on peut conclure qu'ils n'ont, pour la plupart, pas varié en direction pendant cette époque de l'année et qu'ils viennent du SSW (environ 190°).

6) Des observations de « clicks » d'avril à juillet, on peut déduire au contraire que leurs sièges se sont progressivement déplacés dans le sens des aiguilles d'une montre, bien qu'ils fussent généralement indistincts depuis la mi-juin. Le fait qu'ils sont indistincts semble indiquer que le siège des « clicks » est très étendu en été.

Les résultats des observations sont représentés dans le tableau I, les lectures de directions étant exprimées en degrés mesurés dans le sens des aiguilles d'une montre à partir du Nord.

TABLEAU I. — *Direction des atmosphériques à Isohama.*

	Direction des « grinders »			Direction des « clicks »			Intensité du champ des atmosphériques en µ v/m.
Date (1).	Moy.	Max.	Min.	Moy.	Max.	Min.	
Février 5	191	193	189	Pas observés.			
» 21	191	197	186	Id.			
» 23	189	192	180	Id.			
» 26	190	193	187	Id.			
» 28	191	193	190	Id.			
Moyenne =	190						« Grinders » = 33 « Clicks » = 1,198
Mars 3	190	192	187	Pas observés.			
» 7	187	193	183	Id.			
» 9	185	202	169	Id.			
» 16	185	198	179	Id.			
» 24	188	205	170	Id.			
» 31	189	203	181	Id.			
Moyenne =	187						« Grinders » = 26 « Clicks » = 1,209
Avril 7	192	195	188	202	205	199	
» 14	191	195	186	215	225	199	
» 15	192	204	185	243	255	230	
» 16	191	194	189	225	230	220	
» 21	192	194	189	234	240	227	
» 28	192	193	190	232	245	225	
Moyenne =	192		Moyenne =	225			« Grinders » = 42 « Clicks » = 2,050
Mai 5	191	193	189	225	250	205	
» 12	192	193	190	237	251	223	
» 19	191	193	189	236	247	225	
» 23	192	194	190	235	245	227	
» 26	191	193	189	235	245	224	
Moyenne =	192		Moyenne =	234			« Grinders » = 52 « Clicks » = 1,430
Juin 2	191	195	185	237	253	222	
» 9	191	196	188	239	252	225	
» 23	193	202	189	pas de directions distinctes.			
» 27	191	200	185	250	254	245	
» 30	191	200	185	pas de directions distinctes.			
Moyenne =	191		Moyenne =	242			« Grinders » = 55 « Clicks » = 3,100
Juillet 7	190	199	180	directions vagues.			
» 14	192	198	183	id.			
» 21	193	196	190	id.			
» 23	—	—	—	300	vague.		
» 28	192	194	190	270	280	260	
» 30	—	—	—	270	280	260	
Moyenne =	192		Moyenne =	280			« Grinders » = 59 « Clicks » = 3,080

(1) Temps Central Standard du Japon.

Pour fixer les idées, les intensités du champ des atmosphères en microvolts par mètre sont indiquées au tableau I. Les intensités ont été mesurées tous les jours avec une antenne verticale différente et les nombres donnés dans la table sont les valeurs moyennes mensuelles (longueur d'onde = 15,600 m.) mesurées à 16 h. 30, heure à laquelle on peut considérer les atmosphériques comme prédominants. Les intensités des « grinders » sont celles de signaux qui seraient exactement lisibles à travers les « grinders », tandis que les intensités des « clicks » sont celles de « clicks » artificiels qui peuvent être considérés comme égaux en intensités aux « clicks » naturels.

VI. *Résultats obtenus par d'autres observateurs.* — M. T. Nakagami du Ministry of Communications, Tokyo, a traité dans un de ses travaux des directions d'atmosphériques observés au poste d'Osaka en 1924-1925; les résultats sont résumés au tableau II, dans lequel les directions sont exprimées en angles mesurés en degrés dans le sens des aiguilles d'une montre à partir du Nord.

Tableau II. — *Directions des Atmosphériques à Osaka.*

Mois.	Direction des Atmosphériques à 18,000 m.
Janvier	220° (S W)
Février	200° (S S W)
Mars	200° (S S W)
Avril	250° (WSW)
Mai	315° (N W)
Juin	350° (N N W)
Juillet	30° (N N E)
Août	45° (N E)
Septembre	40° (N N E)
Octobre	20° (N N E)
Novembre	300° (WNW)
Décembre	240° (WSW)

J. Obata de l'Aeronautical Research Institute, Tokyo Imperial University, a aussi effectué quelques observations d'atmosphériques à Tokyo, qui furent commencées en juillet 1926, et dont les résultats sont donnés par le tableau III.

Tableau III. — *Directions des Atmosphériques à Tokyo.*

Mois.	Direction des Atmosphériques à 10,000 m.
Août 1926	N W — W N W
Juillet 1926	N W — W N W W N W — W
Septembre 1926	W N W — W
Octobre 1926	S S W
Novembre 1926	S S W
Décembre 1926	Pas observés.
Janvier 1927	S S W
Février 1927	S S W
Mars 1927	S S W — S W
Avril 1927	Pas observés.
Mai 1927	Pas observés.
Juin 1927	W

Nous allons à présent comparer les résultats obtenus par les deux observateurs cités ci-dessus aux nôtres. D'après leurs observations, les positions commençaient à tourner dans le sens des aiguilles d'une montre à partir du SSW en hiver, arrivaient au point le plus éloigné en été et revenaient alors en sens inverse des aiguilles d'une montre jusqu'à ce que la position d'origine fût atteinte à nouveau en hiver, un cycle complet étant parcouru en un an. Dans nos observations, les positions des « clicks » ont décrit une variation analogue, tandis que celles des « grinders » sont restées invariables. Ce fait indique probablement que les deux séries d'observations mentionnées ci-dessus se rapportaient aux « clicks ».

VII. *Conclusions.* —. Des résultats d'observations mentionnées ci-dessus, on peut tirer les conclusions suivantes :

1) On n'a pas trouvé de variation appréciable dans les directions d'atmosphériques observées par la variation des longueurs d'onde qui sont comprises entre 10,000 et 20,000 mètres.

2) Sur les observations diurnes de 7 à 21 heures, on n'a noté aucune variation régulière appréciable dans les directions d'atmosphériques observées pendant le jour. Bien que quelques variations attendues aient, en général, été constatées au lever du soleil, au coucher et pendant la nuit, elles n'ont pas été

aussi remarquables que dans le cas des observations de signaux.

3) Les directions des « clicks » ont été très différentes de celles des « grinders », ce qui semble indiquer que le siège des « clicks » diffère de celui des autres.

4) La variation diurne a été beaucoup plus grande dans les directions des « clicks » que dans celles des « grinders ». Cela paraît indiquer que le siège des « clicks » se trouve plus près de la région observée que celui des « grinders », si on peut supposer que les sièges des deux sortes d'atmosphériques ont des aires à peu près égales. A en juger par les grandes différences dans les intensités, ainsi que dans la nature des sons, cette hypothèse peut être considérée comme vraisemblable.

5) Les directions des « clicks » se sont déplacées depuis le Sud approximativement vers l'Ouest pendant que la saison évoluait d'hiver en été, celles des « grinders », au contraire, étant restées invariables.

6) En été, les « clicks » qui semblent être originaires des régions montagneuses du Japon, paraissent les plus prépondérantes, alors que les « grinders » viennent probablement toujours d'une région tropicale comme les Indes Orientales.

Nous devons exprimer notre reconnaissance envers MM. K. Ichikawa et A. Moriyasu, attachés à ce laboratoire pour leur aide précieuse dans les recherches expérimentales.

Août 1927.

Bruxelles. — Imp. Hayez rue de Louvain, 112.

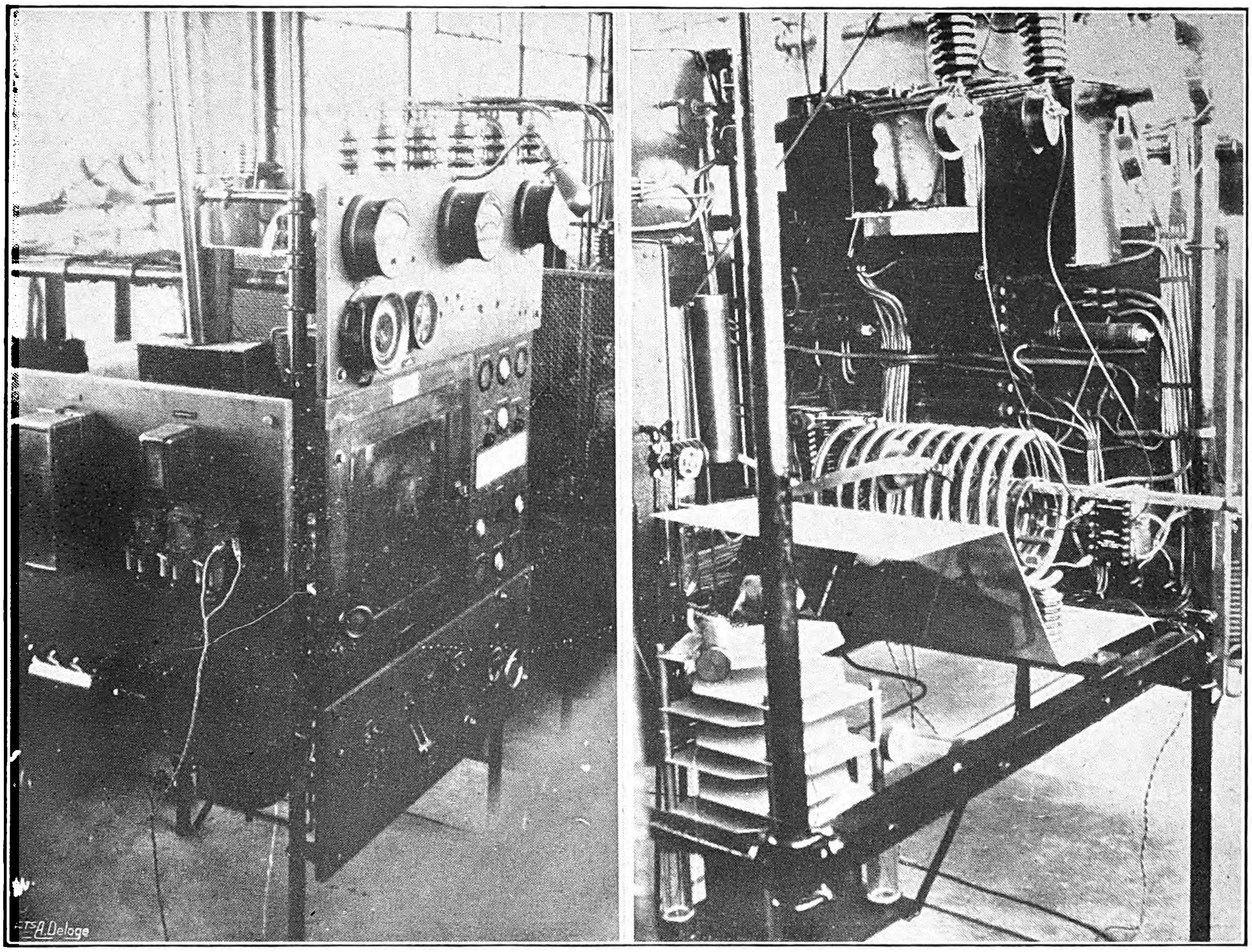


FIG. 3. — Vues des appareils d'émission.

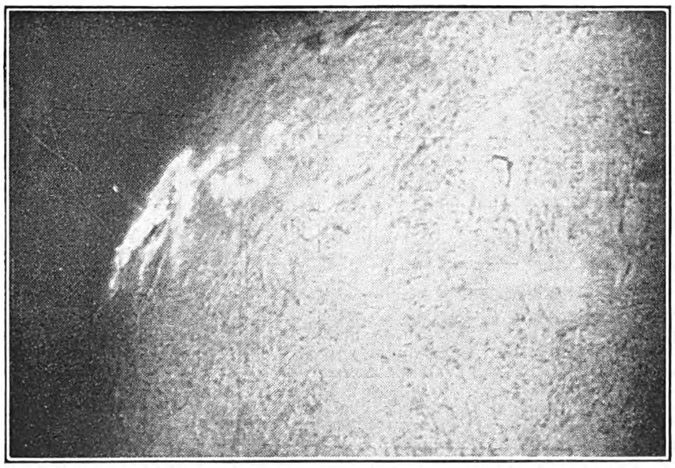


FIG. 6. — Image Hα d'un groupe de taches solaires photographié au Mont Wilson, le 19 août 1927, 14 h. 44 m., temps moyen de Greenwich.

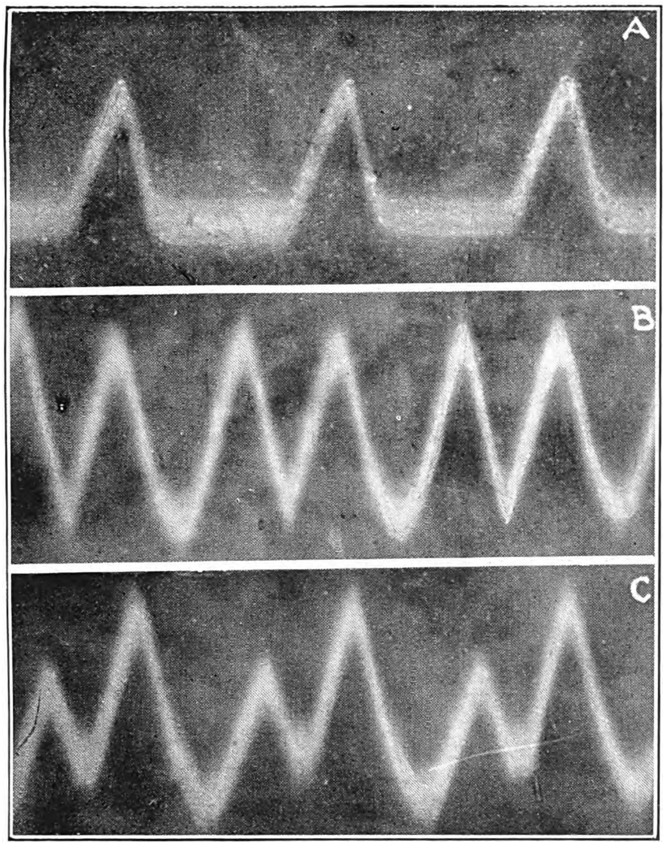


FIG. 4. — Formes d'ondes observées le 25 août 1927.

[(A) Réflexion absente montrant la forme d'onde de l'émetteur. — (B) Onde réfléchie approximativement aussi forte que l'onde de sol. — (C) Onde réfléchie plus faible que l'onde de sol.]

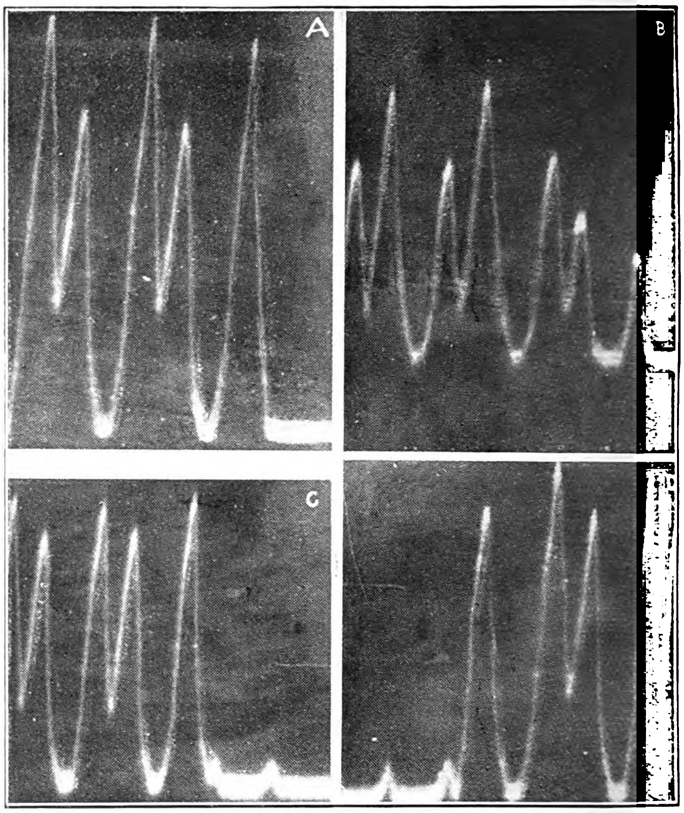


FIG. 5. — Formes d'ondes observées le 8 octobre 1927.

[(A) Fin du tracé, onde réfléchie plus forte que l'onde de sol, pas de fermeture du manipulateur (key-click). — (B) Fin du tracé, onde réfléchie plus petite que l'onde de sol, dernier ressaut de l'onde de sol plus faible que les précédents ainsi que sa réflexion, la proportion restant conservée. — (C) Fin du tracé, onde réfléchie légèrement plus grande que l'onde de sol; à noter le petit coup du manipulateur donnant des ressauts réduits pour l'onde de sol et l'onde réfléchie; l'intervalle de temps entre les petits coups reste le même qu'entre les grands ressauts. — (D) Début du tracé : le coup du manipulateur arrive le long du sol plut tôt que par réflexion; ensuite grand ressaut de l'onde de sol, sa réflexion, etc.; la séparation se termine comme en (C). A noter que l'amplitude du ressaut de l'onde de sol est constante partout.]

G. FERRIE and R. JOUAUST, pp. 12-19.

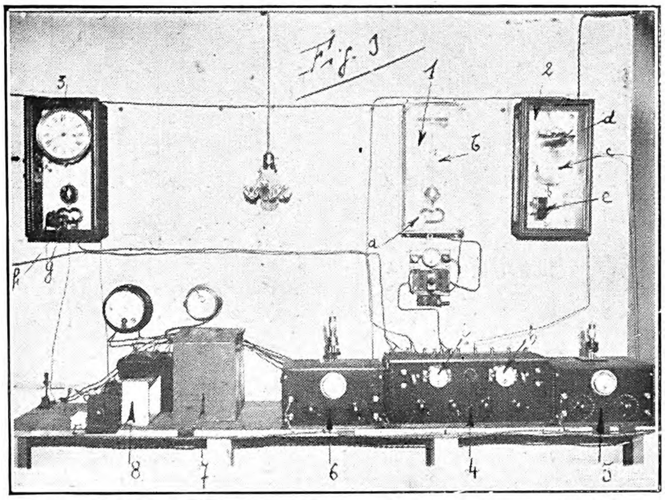


Fig. 3.

1. Self-maintained free pendulum.
    - *a*) Maintaining coil.
    - *b*) Mirror.

2. Control.

    Photoelectric cell (*c*).

    Luminius source (*d*).

    Shutter (*e*).

3. Counting-clock with its self-maintaining coil (*f*), and synchronising coils (*g*).

4. Amplifier for the photoelectric current.
    - *h*) Milliameter for the driving current of the free pendulum.
    - *i*) Milliameter for the synchronising current of the counting-clock.

5. Unit supplying the cell voltage from the alternating current.

6. Unit supplying the plate voltage of the amplifier tubes from the alternating current.

7. Accumulators for the filaments of the tubes of (4) and for the operation of shutter (*e*).

8. Device for continuously charging the batteries from the alternating current.

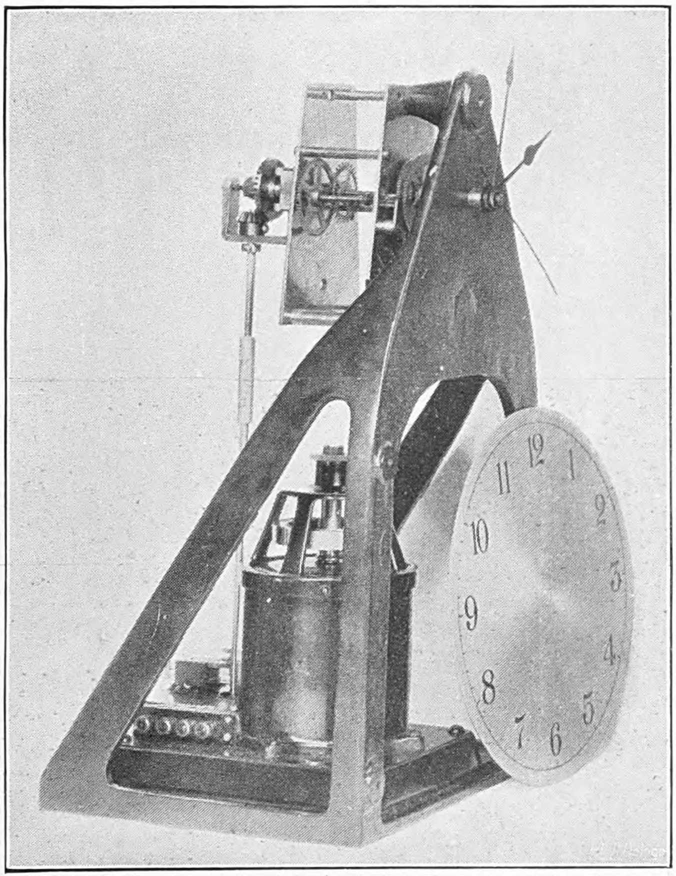


FIG. 1. — Moteur synchrone à 100 cycles engrené sur le mécanisme d'horlogerie.

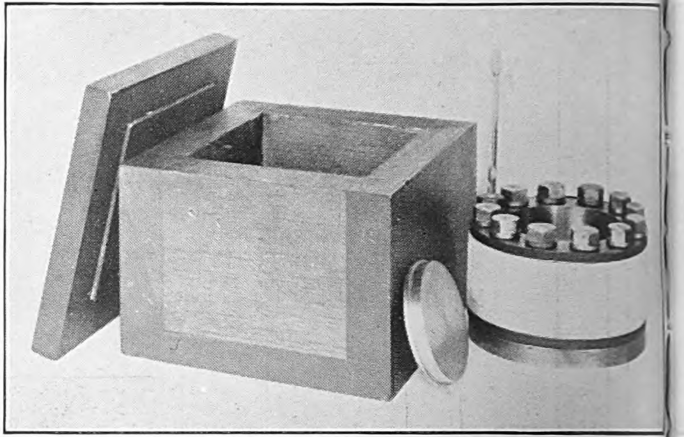


FIG. 7. — Thermostat pour la lame piézo-électrique.

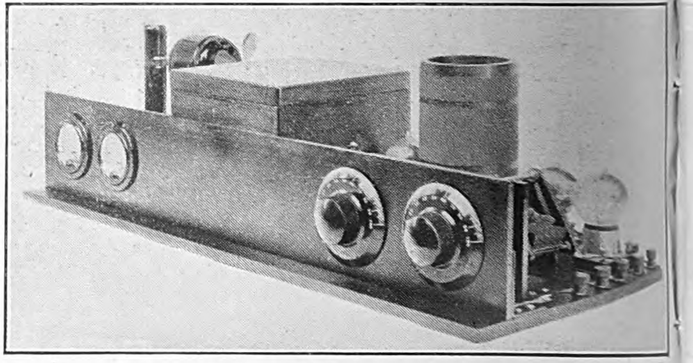


FIG. 8. — Oscillateur étalon à 50,000 cycles.

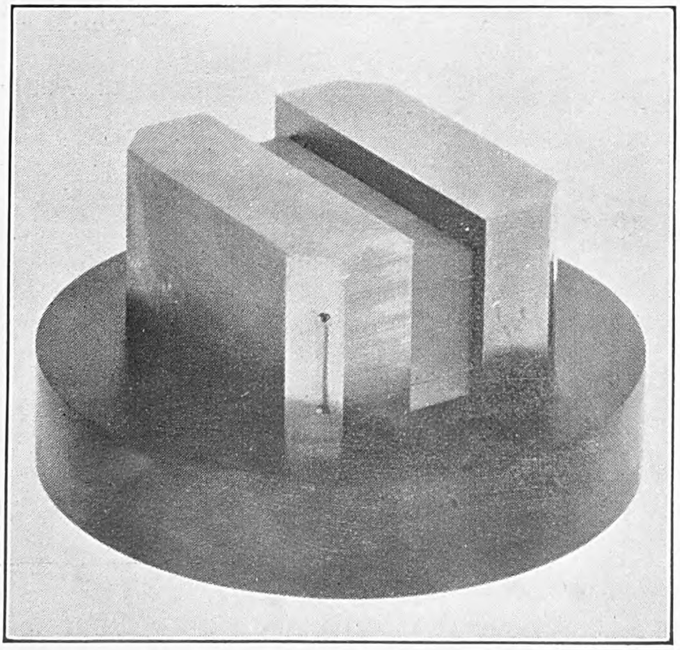


FIG. 6. — Lame de quartz piézo-électrique résonnante, dans son montage.

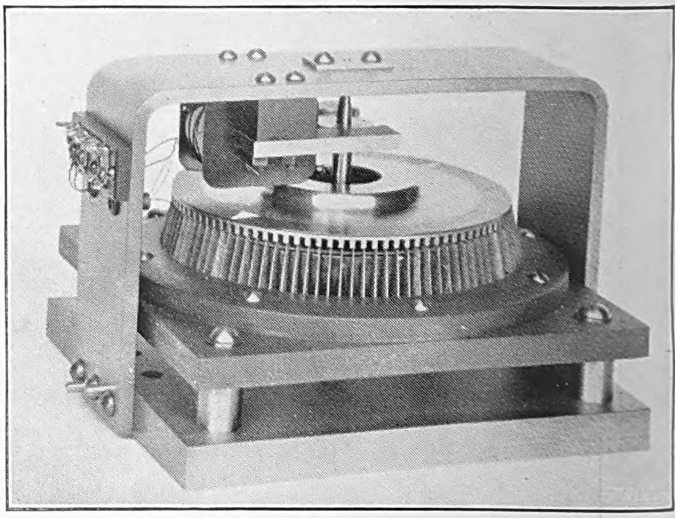


FIG. 11. — Moteur synchrone à 5,000 cycles.

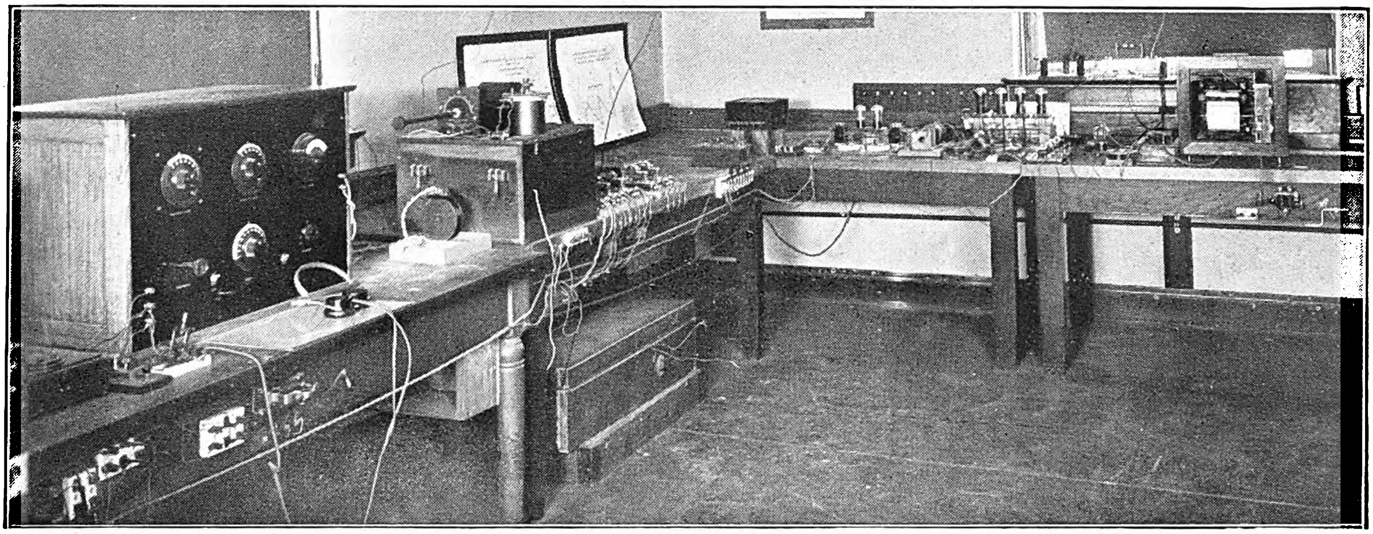


FIG. 2.

George RODWIN et Theodore A. SMITH, pp. 52-57.

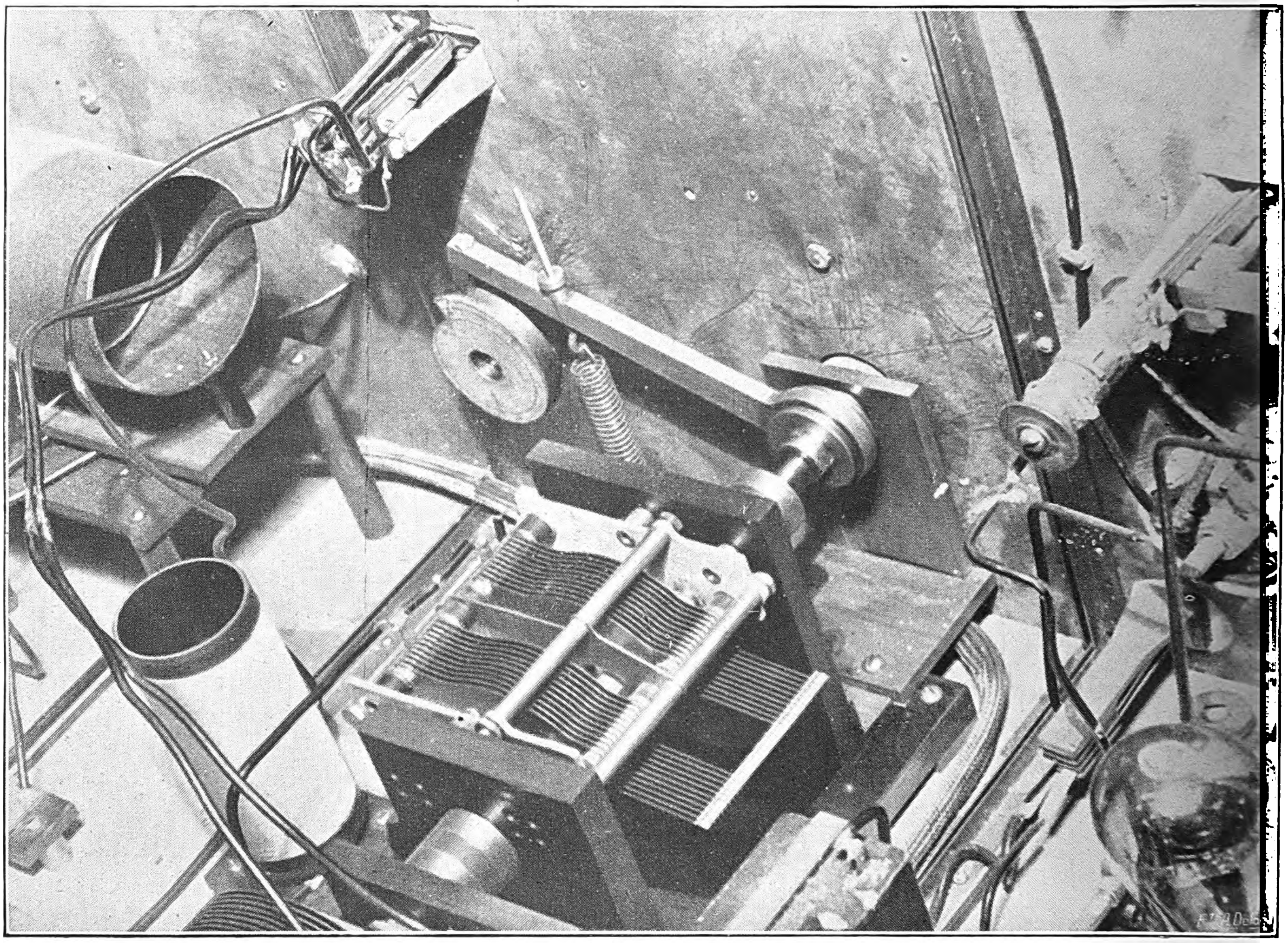


Fig. 8.

Vue de la came et du prin cipal condensateur d'accord.

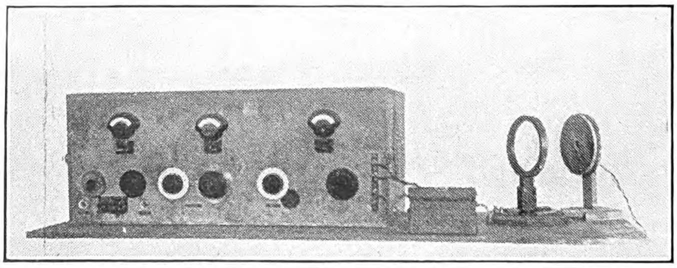


Fig. 9.

Aspect extérieur de l'appareil complet.

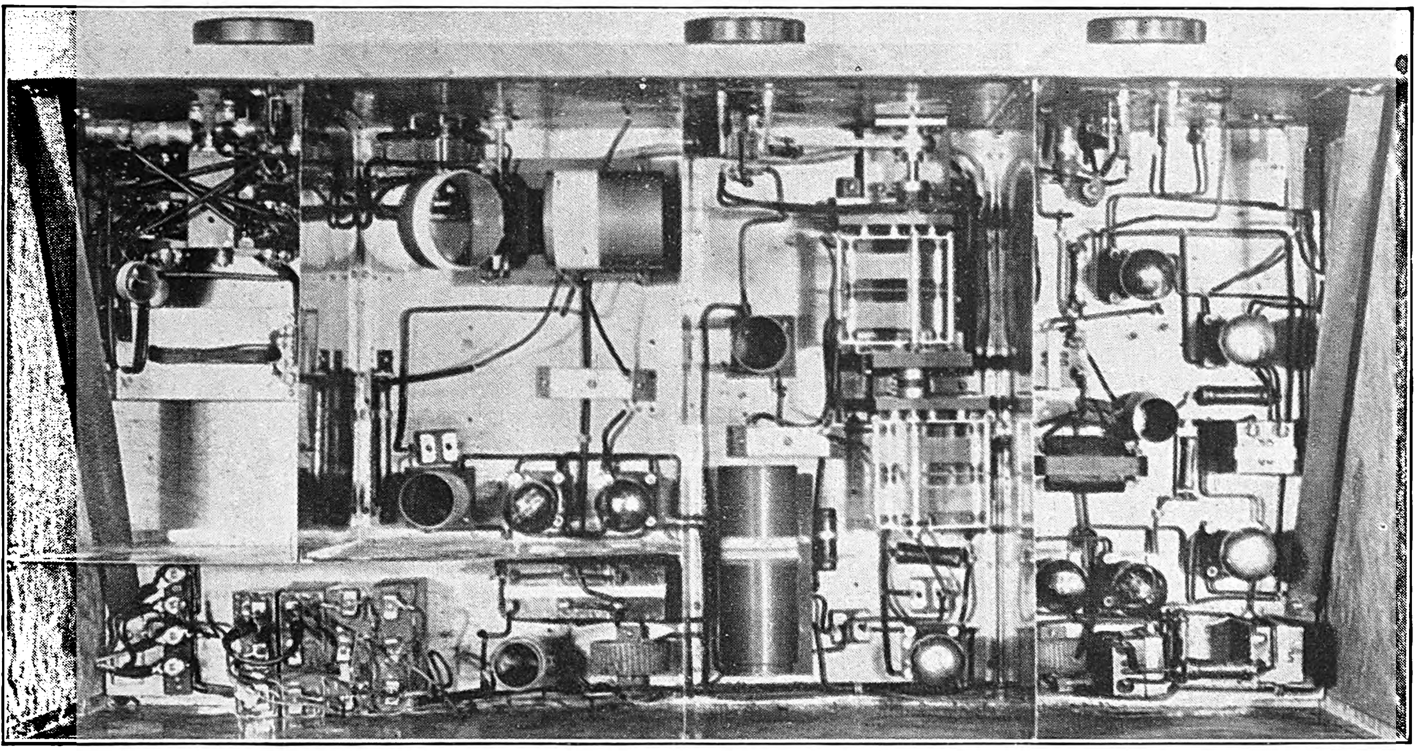


FIG. 10.

Aspect intérieur de l'appareil complet.

ROBERT H. WORRALL et RAYMOND B. OWENS, pp. 62-69.

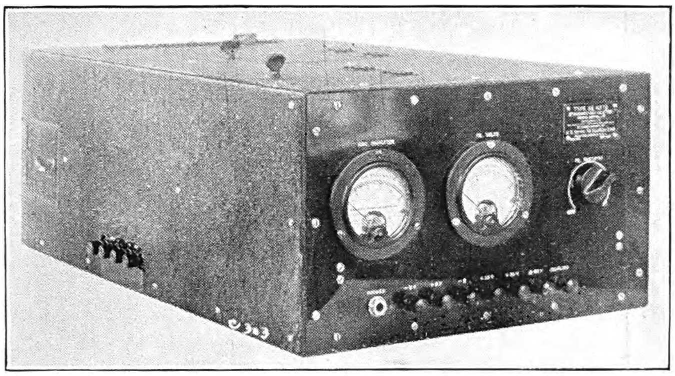


FIG. 5.

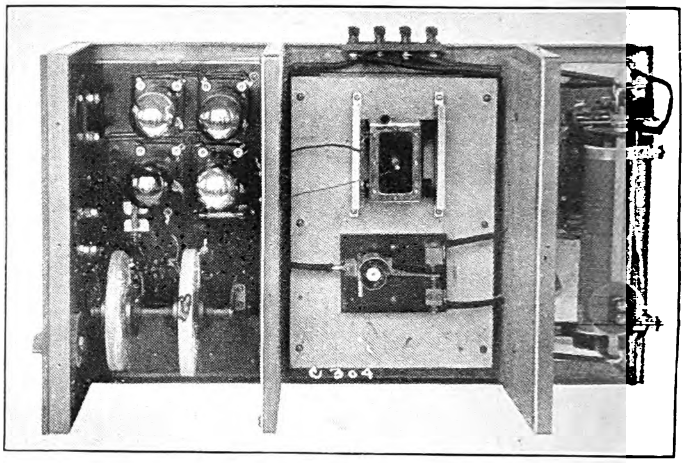


FIG. 6.

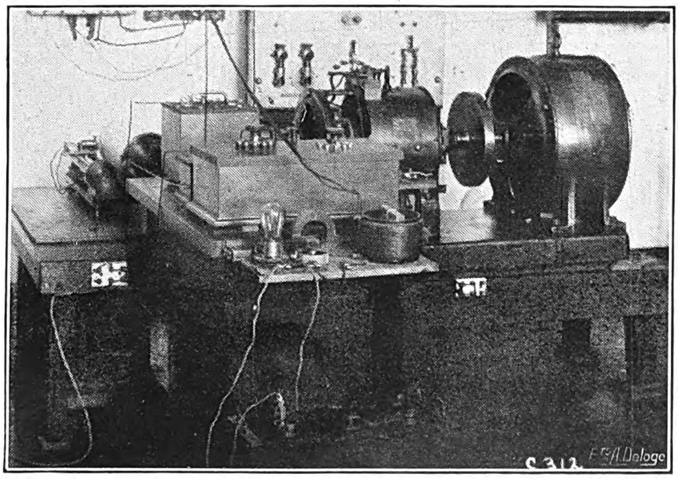

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FIG. 7.

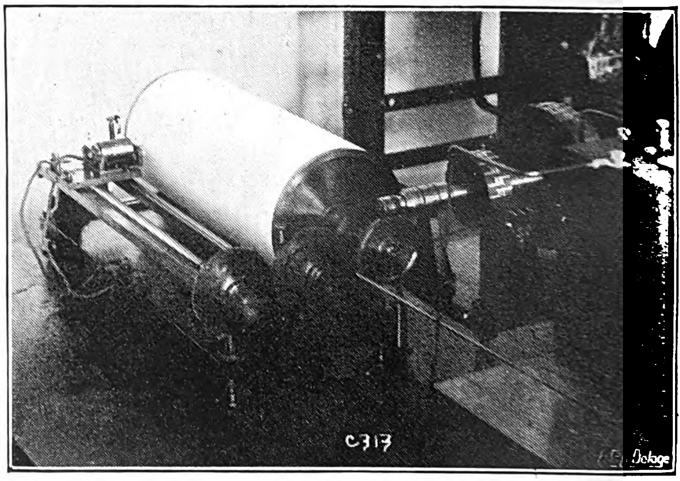
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FIG. 8.

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U.R.S.I.

RECUEIL DES TRAVAUX DE l'Assemblée Générale tenue à Washington en Octobre 1927	PAPERS OF THE General Assembly held in Washington in October 1927

FASCICULE 2

COMPTES RENDUS DE L'ASSEMBLÉE GÉNÉRALE (TEXTES ORIGINAUX)	MINUTES OF THE GENERAL SESSION (ORIGINAL TEXTS)

PUBLIÉ PAR LE SECRÉTARIAT GÉNÉRAL DE L'U. R S. I.
Palais des Académies
BRUXELLES
—
1928

TTFA

Juillet
July 1928 VOL. I Fascicule 2

Union Internationale de Radiotélégraphie Scientifique

International Union of Scientific Radio Telegraphy

PALAIS DES ACADÉMIES, BRUXELLES

Secrétariat Général : 54, avenue des Arts, Bruxelles

Recueil des Travaux de l'Assemblée Générale tenue à Washington en Octobre 1927

Papers of the General Assembly held in Washington in October 1927

COMPTES RENDUS DE L'ASSEMBLÉE GÉNÉRALE

(Textes originaux)

MINUTES OF THE GENERAL SESSION

(Original Texts)

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INTRODUCTION

L'Assemblée générale de l'U. R. S. I. tenue à Washington en octobre 1927 comprenait, outre l'Assemblée générale proprement dite, des séances des quatre Commissions de l'U. R. S. I. et une séance publique. Les comptes rendus de ces dernières réunions font l'objet des fascicules 1, 3, 4, 5 et 6.

Ce fascicule 2 est consacré aux comptes rendus des séances générales. On y trouvera, outre le discours inaugural du Président, le rapport technique du Secrétaire Général relatif à l'activité de l'U. R. S. I. ainsi que la liste des documents scientifiques mis en circulation par les soins du Secrétariat Général.

The General Assembly of the U.R.S.I. held in Washington in October 1927, included, besides the General Assembly properly so called, meetings of the four scientific Commissions of the U. R. S. I. and one public session. The minutes of these last meetings are the object of Fascicules 1, 3, 4, 5 and 6.

Fascicule 2 includes the minutes of the general meetings. In addition to the President's opening speech, the General Secretary's technical report concerning the activity of the U. R. S. I. and the list of scientific documents distributed by the Secretariat General, will also be found therein.

Voici le programme des séances de l'Assemblée générale :

10 octobre : Séance générale.
11 id. : Commission de la Propagation.
12 id. : Commission des Mesures et Etalonnages.
13 id. : Séance publique.
14 id. : Commission des Perturbations atmosphériques.
17 id. : Commission de la Propagation.
17 id. : Commission de Liaison.
18 id. : Commission des Mesures et Etalonnages.
18 id. : Commission des Perturbations atmosphériques.
20 id. : Commission des Perturbations atmosphériques.
20 id. : Commission de Liaison.
24 id. : Commission des Mesures et Etalonnages.
26 id. : Commission de la Propagation.
26 id. : Commission de Liaison.
27 id. : Commission des Perturbations atmosphériques.
28 id. : Séance générale.

Les séances générales ainsi que la séance publique étaient présidées par le Général Ferrié, Président de l'Union.

Lors de la séance d'ouverture du 10 octobre, M. Vernon Kellogg, Secrétaire du Conseil National de Recherches des Etats-Unis, souhaita la bienvenue aux délégués de l'U. R. S. I.

Le Président du Comité national des Etats-Unis, sous les auspices duquel était organisé le Congrès, est le Dr. L. W. Austin, du « Bureau of Standards » de Washington.

La liste des délégués officiels des pays adhérant à l'U. R. S. I. était la suivante :

The following is the programme of the meetings of the General Assembly :

October 10th : General Session.
» 11th : Commission II : Wave Propagation.
» 12th : Commission I : Radio Measurements.
» 13th : Public Session.
» 14th : Commission III : Atmospherics.
» 17th : Commission II : Wave Propagation.
» 17th : Commission IV : Cooperation.
» 18th : Commission I : Radio Measurements.
» 18th : Commission III : Atmospherics.
» 20th : Commission III : Atmospherics.
» 20th : Commission IV : Cooperation.
» 24th : Commission I : Radio Measurements.
» 26th : Commission II : Wave Propagation.
» 26th : Commission IV : Cooperation.
» 27th : Commission III : Atmospherics.
» 28th : General Session.

Both the general and public sessions were presided at by General Ferrié, Chairman of the Union.

At the opening session of October 10th, Mr Vernon Kellogg, Secretary of the National Research Council of the United States welcomed the delegates of the U. R. S. I.

The President of the National Committee of the United States, which organized the Congress, is Dr. L. W. Austin, of the Bureau of Standards of Washington.

The list of official delegates of the States adhering to the U. R. S. I. included :

Australie : Profr J. P. V. MADSEN.
Belgique : Profr R. B. GOLDSCHMIDT.
France : MM. le Général FERRIÉ; le Directeur P. BRENOT; le Dr. LE CORBEILLER; Commandant JULLIEN; le Profr MESNY.
Great Britain : Prof. E. V. APPLETON, F. R. S.; Dr D. W. DYE, B. S.; Mr. H. SHAUGHNESSY.
Italie : Profr VANNI.
Japan : Captain T. MINOHARA; Prof. E. YOKOYAMA.
Norvège : Mr. H. PETERSEN.
Pays-Bas : Prof. B. VAN DER POL, D. Sc.
U.S. of America: Dr. L. W. AUSTIN; Dr. J. H. DELLINGER; Prof. A. E. KENNELLY; Maj. Gen. Charles Mck SALTZMAN; Maj. Gen. George O. SQUIER.

Outre ces délegués, assistaient encore à l'Assemblée générale :

Besides these delegates, the following people were also present at the General Assembly :

Allemagne :	Dr. H. HARBICH.
Canada :	Major W. A. STEELE; Mr. J. W. BAIN.
France :	Capitaine BION; Capitaine BUREAU.
Great Britain :	Capt. P. P. ECKERSLEY; Capt. A. L. HARRIS.
Indes :	Mr. T. G. EDMUNDS.
Irlande :	Mr. T. S. O'MUINEACHAIN.
Japan :	Messrs. T. NAKAGAMI; S. INADA.
Suisse :	Mrs. E. NUSSBAUM; H. EGGLI.
U.S. of America:	Mr. E. F. W. ALEXANDERSON; Dr. R. BOWN; Dr. G. BREIT; Major W. R. BLAIR; Miss M. A. BROWER; Prof. W. G. CADY; Messrs. F. W. DUNMORE; O. DAHL; Dr. H. T. FRIIS; Dr. E. O. HULBURT; Mr. E. L. HALL; Dr. A. HUND; Messrs. J. W. HORTON; V. E. HEATON; E. B. JUDSON; Dr. C. B. JOLLIFFE; Messrs. S. S. KIRBY; F. A. KOLSTER; G. W. PICKARD; T. PARKINSON; H. PRATT; Dr. G. C. SOUTHWORTH; Dr. A. H. TAYLOR; Prof. E. M. TERRY; Dr. TUVE; Dr. W. WILSON; Dr. L. P. WHEELER; Miss I. J. WYMORE.

OPENING SESSION, OCTOBER 10, 1927

About 35 persons were present.

The Chairman of the American Section, Dr. L. W. Austin, called on Mr. Vernon Kellogg, Secretary of the National Research Council, to open the meeting. Mr. Kellogg gave a speech of welcome in which he expressed appreciation of the this Union.

Dr. Austin then called upon the President of the Union, General Ferrié, to take the chair. **General Ferrié** opened the meeting with the following statement :

Messieurs,

Mon premier devoir est d'exprimer la vive gratitude de notre Union envers le Gouvernement américain pour l'accueil qu'il a bien voulu nous réserver et pour la très belle installation qu'il a mise à notre disposition pour nos réunions. Je remercie également en votre nom nos collègues Américains de toutes les amabilités qu'ils nous ont prodiguées et de toute la peine qu'ils prennent pour notre agrément et nos commodités, et en particulier MM. Austin et Dellinger, qui ont préparé notre Assemblée Générale.

Les circonstances n'avaient pas favorisé les Assemblées Générales de notre Union depuis sa fondation en 1919 jusqu'à ce jour. Seules des réunions réduites avaient pu être faites et notre Assemblée actuelle est en réalité la première qui ait pu grouper un nombre important d'adhérents. Cela ne nous a pas empêchés de travailler, il est vrai, et même de bien travailler, mais il était devenu néanmoins indispensable d'examiner de nouveau dans leur ensemble nos travaux et notre organisation, et de pouvoir échanger verbalement nos idées sur toutes les questions qui nous intéressent.

La nature même de ces questions justifie l'existence de notre groupement, et aucune autre Union Internationale ne nécessite au même degré que la nôtre l'exécution de travaux en commun. Les résultats que ceux-ci nous ont déjà donnés ont une importance indéniable. Il nous a été possible en outre de faire l'épreuve de nos méthodes de travail; aussi n'aurons-nous pas trop de peine, j'espère, à établir le programme des études à entreprendre ou à continuer.

Examinons maintenant brièvement les résultats de l'activité scientifique de notre Union, depuis l'Assemblée Générale de Bruxelles en 1922.

Mesures. — La mesure précise des fréquences est une des questions les plus importantes pour les spécialistes de la radio. Il est évidemment nécessaire, d'une part, de créer des méthodes de mesures ayant une haute précision et, d'autre part, d'établir un accord parfait entre les étalons de fréquence des divers laboratoires chargés d'effectuer la vérification des appareils servant à contrôler les fréquences des oscillations employées par les postes radios émetteurs.

Sur l'initiative du Bureau of Standards, les laboratoires intéressés commencèrent, il y a déjà trois années, par effectuer simultanément la mesure des fréquences des émissions de divers postes. La difficulté d'opérer exactement au même instant, et au milieu de brouillages, ne permit pas de tirer de cette comparaison toute la précision qu'on en attendait.

La possibilité, signalée pour la première fois par CADY, d'utiliser les propriétés piézo-électriques du quartz pour en faire un étalon de fréquence, vint heureusement donner une remarquable solution du problème de la comparaison des fréquences.

La constitution d'étalons de cette espèce et l'étude de leur fonctionnement ont donné lieu déjà à de nombreuses recherches de laboratoire parmi lesquelles il faut signaler en particulier celles du Bureau of Standards (HUND) et celles du National Physical Laboratory (DYE). Un travail inédit de M. Tawil, sur une nouvelle manière d'utiliser les quartz pour ces mesures, a été exécuté sous les auspices du Comité français de l'U. R. S. I. et sera communiqué à la Commission des mesures. Des expériences démonstratives seront exécutées devant elle.

L'échange de ces étalons de fréquence en quartz entre les laboratoires spécialisés a montré l'existence entre ceux-ci d'un accord de l'ordre de quelque dix-millièmes. Il est même probable que les très petites différences constatées disparaîtront quand nos procédés de mesure auront été encore améliorés.

La mesure de la vitesse de propagation des ondes n'a pas donné lieu à de nombreuses recherches pendant ces dernières années. Toutefois, il convient de signaler les travaux faits à ce sujet par M. MOSAO HASIMOTO et la proposition soumise à l'Union par le Comité Japonais et que la Commission de mesures aura à examiner. Des travaux de ce genre seront prochainement entrepris en France également.

Propagation. — L'étude de la propagation des ondes est certainement la plus importante pour notre Union, à l'époque actuelle tout au moins. Elle a suscité de très nombreux travaux, relatés notamment dans les documents distribués par notre Secrétariat Général à tous les Comités Nationaux. L'activité de notre Union n'a rien laissé à désirer à ce point de vue.

On avait tout d'abord décidé de faire porter les observations sur des signaux spéciaux émis dans ce but par diverses stations radios, mais il semblera probablement inutile de les maintenir dans l'avenir, car il suffirait d'observer les émissions commerciales de mêmes stations. Notre Commission de propagation aura à se prononcer à ce sujet.

Nous n'avions guère envisagé, d'autre part, jusqu'à maintenant que l'emploi d'ondes longues pour cette étude dont le but avait pu être defini de la manière suivante : chercher une relation entre les phénomènes solaires et météorologiques et le champ produit à grande distance par une émission. De nombreuses mesures ont été effectuées, notamment par MM. SMITH-ROSE, HOLLONG-WORTH, BARFIELD (en Angleterre), AUSTIN (aux États-Unis), MESNY (en France), et sont encore poursuivies chaque jour, à différentes heures, sur le champ produit par des émissions lointaines à ondes longues. Elles devront être continuées pendant la durée d'un cycle solaire tout au moins, si l'activité solaire joue un rôle dans les phénomènes de propagation, comme on le suppose.

Déjà AUSTIN et GREENLEAF PICKARD (Etats-Unis) semblent avoir mis en évidence certaines relations entre l'intensité des transmissions et les phénomènes magnétiques. APPLETON, ESPENSCHIED, ANDERSEN et BUDLEY (Angleterre) ont aussi signalé des résultats analogues. Les mesures faites en France, en revanche, n'ont, par contre, pas permis de tirer des conclusions affirmatives.

AUSTIN a pu mettre en évidence par ailleurs une relation entre la température et les phénomènes de transmissions, qui confirmerait certaines hypothèses émises en France par Bureau.

Un nouvel essor a été donné à ces recherches, qui progressaient bien lentement, par l'usage des ondes courtes, qui s'est considérablement développé depuis deux ans environ et qui a permis de réaliser des portées considérables en ne mettant en jeu que de faibles énergies, notamment avec des longueurs d'ondes comprises entre 15 et 50 mètres. Ces résultats inattendus ne peuvent, en effet, être expliqués qu'en faisant intervenir une couche fortement ionisée et située dans la haute atmosphère, hypothèse déjà émise il y a un assez grand nombre d'années par Heaviside en Angleterre et Kennelly aux États-Unis.

Le rôle de cette couche a donné lieu à de nombreuses recherches théoriques dues notamment à Eccles, à Nichols et Schelleng, à Halburth et Taylor, à Nagaoka, à Larmor, à Boeker et Rice, à Lasson, à Eckersley. Comme il ne suffit pas de faire des hypothèses et qu'il est nécessaire de les comparer aux résultats de l'expérience. de nombreux membres de notre Union se sont employés à ces comparaisons.

En premier lieu, il convient de citer les belles expériences d'Appleton et de ses collaborateurs, qui semblent mettre nettement en évidence l'existence de cette couche et avoir permis de mesurer sa hauteur. Les travaux de Breit et Treise, aux États-Unis, ont confirmé ces résultats.

Les phénomènes d'évanouissement (fading) peuvent également nous donner de précieux renseignements sur les phénomènes de propagation. Les travaux de Dellinger à ce sujet doivent être cités en particulier.

Des recherches spéciales concernant la propagation des ondes courtes ont été aussi entreprises dans divers pays, en France notamment, où elles ont été préparées par l'Office national météorologique sous les auspices du Comité français de l'U. R. S. I., et exécutées en utilisant des émissions de stations fixes, de navires traversant l'Atlantique et de ballons-sondes.

Le phénomène des zones de silence a fait aussi l'objet de recherches importantes aux États-Unis, dont l'étendue permet de les étendre facilement à de grandes distances du point de production des ondes. Une coopération des États européens est nécessaire pour que des études analogues soient possibles sur l'ancien continent. Aussi le Comité français a-t-il décidé de présenter un vœu dans ce but à l'examen de notre Commission de liaison.

La coopération de purs physiciens doit, d'autre part, être recherchée par notre Union, pour tâcher de pénétrer plus avant dans la connaissance de la nature de la couche d'Heaviside-Kennelly. Déjà le Comité britannique a provoqué devant la Royal Society une très intéressante discussion sur les phénomènes qui se produisent dans la haute atmosphère. Le Comité français a également obtenu qu'une discussion analogue ait lieu devant la Société française de Physique. Enfin, la Commission créée par le Conseil international de Recherches pour l'étude des relations entre les phénomènes solaires et terrestres a demandé que des travaux soient entrepris en commun par les spécialistes de l'astronomie, de la géophysique et de la radiotélégraphie, et a proposé un programme de recherches.

Atmosphériques (Statics). — L'étude des « statics », les grands ennemis de la radiotélégraphie pratique, a fait également l'objet de travaux importants de notre Union. Aux États-Unis, Austin a cherché à déceler leurs lieux d'origine. En Angleterre, Appleton et Herd, en France Bureau, et d'autres encore, ont poursuivi de très intéressantes études sur les causes des statics et sur la distance à laquelle ils se font sentir. Les opinions émises sont souvent contradictoires et leur examen donnera certainement lieu à d'intéressantes discussions devant notre Commission des statics.

Le résumé, très sommaire, que je viens d'exposer et dans lequel je m'excuse d'avoir omis beaucoup de travaux importants, suffit à démontrer que l'activité de notre Union a déjà été très grande et doit encore augmenter dans l'avenir, car aucune solution définitive n'a été apportée aux problèmes qui nous préoccupent.

Peut-être jugerez-vous utile de demander à nos Commissions d'examiner s'il ne serait pas possible d'accroître la coordination entre les divers travaux exécutés par les Comités Nationaux, en tenant compte de l'expérience qu'elles ont déjà acquise dans leur effort à remplir leur tâche le mieux possible.

Nous aurons également à nous occuper, en Assemblée Générale, avant de nous séparer, d'un certain nombre de questions d'organisation. Quelques-unes d'entre elles vous ont déjà été signalées par notre Secrétaire Général.

En premier lieu, nous aurons à examiner s'il ne conviendrait pas d'apporter quelques modifications à nos statuts.

D'autre part, la fixation du montant des cotisations en francs-or au lieu de francs-papier est déjà portée à notre ordre du jour.

Nous aurons également à procéder, avant de nous séparer, au renouvellement du Bureau de l'Union. Permettez-moi de vous dire à ce sujet que je désire être remplacé dans les fonctions de Président de l'Union, que j'ai l'honneur d'occuper depuis trop longtemps.

Je vous propose, mes chers Collègues, de procéder dès aujourd'hui à la constitution des Bureaux de nos Commissions statutaires, pour la durée de notre Assemblée Générale, car les membres des Bureaux officiels ne sont pas tous présents, et de créer en outre la Commission financière prévue par nos statuts, en la chargeant en outre des questions d'organisation. Cette commission pourrait être formée des membres des Bureaux de l'Union et des Commissions scientifiques.

Comme tous les membres ici présents s'intéressent à toutes les questions ressortissant aux diverses commissions scientifiques, je vous propose de décider que les commissions se réunissent aux jours et heures choisis d'un commun accord en dehors des moments des réunions de la Commission Technique de la Conférence radiotelegraphique, mais jamais simultanément, afin que tous nous puissions assister à toutes ces séances.

Avant de prier notre Secrétaire Général de vouloir bien lire son rapport, je tiens à adresser à M. Robert GOLDSCHMIDT les chaleureux remerciements et les félicitations de l'Union tout entière, pour le dévouement remarquable qu'il a sans cesse apporté dans l'exécution de sa tâche si difficile et si lourde. Aucun de nous n'oublie, au surplus, que M. R. Goldschmidt est le fondateur du groupement de spécialistes de la radiotélégraphie qui a été l'origine de notre Union et qu'il a été aussi pour celle-ci un généreux donateur.

Nous devons également de vifs remerciements à M. Paul GOLDSCHMIDT, qui remplit avec un très grand zèle les fonctions de Secrétaire Général adjoint.

At the close of General Ferrié's address, the General Secretary, **Dr. R. B. Goldschmidt**, presented *reports as Secretary and as Treasurer*. These reports are on file.

The President pointed out that of the four International Commissions, the Chairmen of only two were present, namely, Dr. Austin, Chairman of the Commission on Wave Transmission, and Professor Vanni, Chairman of the Commission on Cooperation. It was therefore necessary to select temporary chairmen to preside at meetings of the other two Commissions. Accordingly Dr D. W. Dye was chosen as temporary chairman of the Commission on Measurements and Standard, and Professor R. Mesny, temporary chairman of the Commission on Atmospherics.

The President announced that the sessions of the Commissions would not be held simultaneously but that they would be held one after the other so that all persons might attend every session if desired. The sessions are to meet in the order : II, I, III, IV, to be followed by additional sessions in the same order as long as there remains any business to be done.

A special temporary committee was appointed to study questions of finance and administration. This Committee consisted of the President, General Secretary, Chairman and Technical Secretary of the American Section, and the four Chairmen of the Commissions.

It was announced that the public session for the formal presentation of scientific papers would be held on October 13 in two sessions at 2 : 30 P. M. and : 8 : 00 P. M.

Rapport de M. Robert-B. Goldschmidt, Secrétaire Général de l'U. R. S. I.

MONSIEUR LE PRÉSIDENT, MESSIEURS,

Je vous demande la permission de remercier notre Président, le général Ferrié, pour ses paroles trop élogieuses pour votre Secrétaire Général; elles constituent la plus belle récompense du travail fourni et le meilleur des encouragements.

Pourtant ces éloges retombent en grande partie sur notre Président lui-même, qui a grandement facilité la besogne du bureau de Bruxelles par une inépuisable obligeance et une amabilité d'autant plus charmante qu'elle est naturelle chez lui.

Je remercie notre Président de rendre hommage aussi à tout le personnel du bureau du Secrétariat et principalement à mon homonyme, M. l'ingénienr Paul Goldschmidt, qui, sans avoir aucune parenté avec moi, s'intéresse comme moi tout spécialement au développement de notre Union, auquel il consacre beaucoup de temps et d'efforts.

Je ne voudrais pas commencer mon rapport sans rendre hommage à l'aide que nous avons rencontrée en ce pays. L'esprit de l'U. R. S. I. est personnifié ici par M. le Professeur Austin, qui fut l'animateur de notre Union. Sa science et son esprit d'organisation sont universellement reconnus. C'est un citoyen dont un grand pays peut être fier.

Je voudrais aussi remercier M. Dellinger, le Secrétaire de notre Congrès actuel, pour ce qu'il a fait pour la préparation de notre travail et pour ce qu'il va faire ces prochains jours pour l'U. R. S. I. Il ne ménagera pas ses peines et son travail; ne lui ménageons pas nos remerciements anticipés.

Lors de l'Assemblée Générale qui s'est tenue à Bruxelles, en juillet 1922, les pays repris ci-dessous faisaient partie de l'U. R. S. I. : Angleterre, Belgique, États-Unis, France, Italie, Norwège.

Depuis ce moment, nous avons noté successivement l'adhésion des pays ci-dessous : Australie, Japon, Hollande, Portugal, Union Sud-Africaine.

Aujourd'hui, onze pays font régulièrement partie de l'U. R. S. I.

Par le présent rapport, le Secrétariat Général se propose de donner un aperçu récapitulatif des travaux effectués et en outre de suggérer certaines dispositions complémentaires aux statuts et règlements de l'Union.

Maintenant je vais m'efforcer de vous montrer les défauts et imperfections qui se sont révélés dans le fonctionnement du Secrétariat de l'Union et qui m'amèneront à vous faire quelques propositions. Je vous propose de les écouter en ce moment, mais de ne les discuter qu'à la dernière Assemblée Générale; ainsi vous aurez, Messieurs, le temps d'y réfléchir. Ces différentes propositions sont conditionnées par les ressources actuelles de notre Union.

Si vous approuvez la création de ressources budgétaires nouvelles, ainsi que je voudrais vous le proposer, elles pourront être élargies considérablement.

Travaux de l'U. R. S. I. et circulation des documents.

L'article 6 des statuts prévoit qu'un bureau administratif, sous la direction du Secrétaire Général, assure notamment la préparation et la distribution des publications approuvées par l'Assemblée Générale. — Ce mandat n'est pas prévu en ce qui concerne les documents non approuvés par l'Assemblée Générale.

Le Secrétariat Général a cependant mis en circulation, sous forme de documents numérotés, les publications et résultats des mesures émanant notamment des Comités Nationaux et des Commissions. — Cette décision a été prise dans le but de tenir plus régulièrement les membres des Comités Nationaux et des Commissions au courant des travaux de leurs collègues et de provoquer éventuellement de nouvelles initiatives. Cette distribution a débuté en 1924. Les documents ainsi distribués n'ont pas été répartis comme appartenant plus spécialement à l'une des différentes Commissions, la plupart d'entre eux étant de nature à apporter une contribution aux travaux de plusieurs Commissions simultanément, et en tout cas, à intéresser tous les membres de l'U. R. S. I. — Le Secrétariat Général les a donc compris dans une numérotation unique, se bornant à en indiquer l'origine, et laissant aux Commissions le soin d'adopter une classification appropriée pour les documents qui les concernent plus spécialement.

151 documents numérotés ont été mis ainsi en circulation par le Secrétariat Général.

Vous en verrez l'énumération récapitulative dans le rapport qui vous a été remis ce jour (1).

Nous pouvons être légitimement fiers du travail fait, qui représente en son ensemble une contribution extrêmement importante à l'étude de la T. S. F. et à la compréhension de phénomènes encore très mystérieux.

C'est aussi une contribution sérieuse à l'avancement technique de la T. S. F.

M. le Général Ferrié a bien voulu me dire que le Conseil Général de recherches apprécie nos efforts et considère notre Union comme une section très vivante de son organisation.

Remarques concernant la circulation des documents.

Ainsi que je viens de vous le dire, c'est à partir de l'année 1924 seulement que la circulation des documents a été organisée par le Secrétariat Général, qui, à cet effet, a prié les Comités nationaux de les lui faire parvenir à un nombre suffisant d'exemplaires. Il n'a pas pu être procédé rétroactivement en ce qui concerne les publications des années antérieures, sauf pour les documents relatifs aux caractéristiques et mesures des émissions U. R. S. I.

Le Secrétariat Général ne peut naturellement envisager la réimpression des documents reçus sous la forme imprimée. Il n'a procédé à cette réimpression qu'en ce qui concerne les documents relatifs aux caractéristiques et mesures des émissions U. R. S. I., lesquels ont été envoyés aux Comités Nationaux en nombre suffisant pour que tous leurs membres en possèdent.

D'autres documents ont été envoyés aux Comités nationaux et aux Commisions à raison d'un exemplaire au minimum par Comité national et par Commission.

Pour satisfaire au désir de certains Comités nationaux, qui voudraient recevoir à suffisance les exemplaires de tous les documents, le Secrétariat Général présente la proposition suivante :

Proposition n° 1. — Tous les documents imprimés seront envoyés au Secrétariat Général par le Comité national d'origine à un nombre d'exemplaires fixé à 100. Le Secrétariat Général remboursera au Comité national qui aura fait l'envoi le prix de la moitié des exemplaires fournis, établi au prix coûtant.

Service bibliographique.

Grâce à l'obligeance de M. Dellinger, le Secrétariat Général a pu organiser un service de circulation des renseignements bibliographiques qui paraît avoir été apprécié par les membres.

Réserves faites pour ce qui concerne le mode de fonctionnement actuel, il semble qu'un service bibliographique en matière de radiotélégraphie scientifique, établi sur la base internationale, soit de nature à contribuer au développement de cette science, et rentre essentiellement dans le cadre de l'activité de l'U. R. S. I.

Proposition n° 2. — Le Secrétariat Général propose que l'Assemblée Générale mette en discussion le principe d'un service bibliographique et qu'elle pose définitivement les bases du fonctionnement de ce service.

Actuellement, les fiches bibliographiques envoyées par le Secrétariat Général reproduisent les références publiées par le *Radio Service Bulletin* (Bureau de Navigation, Department of Commerce, Washington) et se rapportent exclusivement à la propagation des ondes et aux mesures.

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(1) Voir plus loin, p. 11. — See p. 11.

Règlement des Commissions.

Les organismes agissants de l'U. R. S. I., tels qu'ils sont prévus par le règlement des Commissions internationales, sont actuellement, en ordre principal, ces dernières, agissant en liaison avec les Comités nationaux.

Cette organisation semble être la plus parfaite pour obtenir des conclusions ayant un caractère international, et, à ce titre, doit être maintenue.

Elle présente cependant l'inconvénient d'être lente, en raison de la difficulté de réaliser l'accord des membres appartenant à plusieurs pays, dont certains sont très éloignés des autres.

Il semble nécessaire, en vue de rendre plus souples et plus fréquents les moyens de documentation réciproque des membres de l'U. R. S. I., d'admettre de nouveaux centres d'activité; à cet effet, il est proposé ce qui suit :

Proposition n° 3. — Les Comités nationaux pourront transmettre directement au Secrétariat Général, en même temps qu'au Président de la Commission intéressée, s'il y en a une, les résultats obtenus par eux ou par les Commissions nationales qu'ils auraient instituées. Le Secrétariat Général transmettra ces résultats aux autres Comités nationaux ainsi qu'au Président de l'U. R. S. I.

Le Bureau de l'U. R. S. I. se mettra éventuellement d'accord avec la Commission internationale compétente pour ce qui concerne la présentation de ces résultats à l'Assemblée Générale.

Rapports partiels des Commissions internationales.

En vue de documenter régulièrement les Comités nationaux sur les travaux effectués par les Commissions internationales, il serait utile que celles-ci envoient chaque année un rapport partiel résumant les résultats acquis par elles.

A cet effet, il est proposé ce qui suit :

Proposition n° 4. — Les Commissions internationales enverront chaque année, au Secrétariat Général, à la date du 1^er^ mars, un rapport partiel où elles consigneront les résutats acquis par elles au cours de l'exercice écoulé. Les rapports partiels seront publiés et envoyés au Comités nationaux trois mois après leur dépôt.

Clôture des enquêtes et des observations.

Lorsque le Secrétariat général est amené à consulter les Comités nationaux ou les Commissions sur une question déterminée, il éprouve des difficultés du fait que certains des organismes consultés ne fournissent pas de réponse et le mettent ainsi dans l'impossibilité de réunir un dossier complet.

Afin que les travaux de l'U. R. S. I. ne s'en trouvent pas entravés, le Secrétariat Général propose ce qui suit :

Proposition n° 5. — Par application de l'article 4 des statuts, le Secrétariat général pourra, à l'invitation d'un Comité national, d'une Commission ou du Bureau, consulter les Comités nationaux et les Commissions et entreprendre auprès de ces organismes des enquêtes sur des questions spéciales.

Il pourra, d'accord avec le Président, tirer les conclusions de ces consultations et de ces enquêtes, pour autant que trois Comités nationaux au moins aient répondu et après un délai qui ne sera pas inférieur à six mois. Ce délai pourra être réduit lorsque les circonstances l'imposeront.

Cependant, le Secrétariat Général, d'accord avec le Président, pourra prendre en considération, par la suite, les éléments nouveaux que fourniraient les Comités nationaux et les Commissions, pour autant que ses conclusions n'auraient pas revêtu un caractère définitif.

Nous devons nous préoccuper de développer notre Union et augmenter nos ressources en amenant des pays n'adhérant pas encore, à faire partie de Union. Il est incroyable que certains pays, comme la Suisse, où il y a tant de techniciens savants, et l'Espagne, pour n'en nommer que deux au hasard, ne fassent pas partie de notre Union. Nous avons en ce moment une excellente occasion d'attirer l'attention des représentants des différents pays sur notre Union.

Nous pouvons aussi obtenir que les pays déjà adhérents augmentent leurs cotisations. Nous devrons faire des démarches pour qu'elles subissent ce que nous appelons une péréquation, c'est-à-dire tiennent compte de la dévalorisation du franc depuis la création de notre Union, et fassent un réajustement; mais nous ne devons pas nous faire d'illusions sur ce que rapportera la contribution des Gouvernements.

Nous pourrons aussi, dès à présent, si l'Assemblée partage cette façon de voir, mettre en circulation des bulletins de souscription pour le volume que nous aurons à faire paraître et qui comportera la publication des travaux approuvés par l'Assemblée Générale et les procès-verbaux de la présente réunion.

La publication de ce volume devra probablement se faire en deux langues, français et anglais. Les documents anglais étant traduits en français et réciproquement.

Cela amènera des frais considérables qu'il convient de couvrir par un budget extraordinaire.

D'autre part, les bulletins de souscription nous fourniront des renseignements intéressants sur le nombre d'exemplaires à imprimer, ce qui est très difficile à établir autrement.

Enfin, il nous paraît nécessaire de prier l'Assemblée Générale de nous permettre de demander des subsides auprès de tous ceux qui peuvent porter un intérêt quelconque à la T.S.F.

On pourrait s'adresser aux grandes compagnies qui fabriquent et utilisent des appareils de T. S. F., aux compagnies de navigation et, en général, à tous les usagers de la T. S. F. et à tous ses admirateurs.

Nous pouvons justifier ces demandes par l'appui que donnent les travaux scientifiques aux travaux techniques et à la T. S. F. pratique.

Si nous avions des ressources en rapport avec notre activité, nous pourrions augmenter considérablement celle-ci : Subventionner les commissions et les chercheurs d'une façon efficace; organiser d'une façon plus complète le secrétariat; créer de nouvelles commissions internationales et spécialement celles qui rapprocheraient les travaux du laboratoire de ceux de l'usine et aider efficacement les techniciens volontaires en les mettant à même de collaborer utilement aux travaux de l'Union.

Je voudrais donc vous demander, dès ce jour, de me permettre d'essayer de recueillir de nouvelles ressources dans tous les pays, avec l'appui des Comités nationaux de ces pays.

A l'occasion de la réunion de l'U. R. S. I. à Washington, je voudrais être autorisé à demander, d'accord avec le Comité américain, des subsides dans ce pays, qui est le plus généreux du monde et le plus largement ouvert aux initiatives qui peuvent amener un progrès.

Si nous réussissions dans nos démarches, — et je suis persuadé que tous ceux qui seront sollicités, connaissant notre but et notre activité passée, répondront à notre appel, — nous pourrons envisager l'avenir de notre Union sous un angle nouveau et espérer réaliser une centrale de la T. S. F. scientifique : la maison internationale de la T. S. F. scientifique.

Notre application, notre activité à tous nous donnent légitimement droit à toutes les ambitions.

Maintenant, pour vous mettre complètement à l'aise, je me permets de vous offrir — comme l'a fait le Général Ferrié et dans le même esprit — ma démission de Secrétaire Général de l'U. R. S. I., pour que vous puissiez en toute liberté, et sans crainte de froisser personne, prendre des décisions au sujet du fonctionnement et du personnel du Secrétariat Général.

Je voudrais demander maintenant à M. le Président de donner la parole au Trésorier.

Mon principe a été celui de tout bon trésorier : de faire rentrer le plus possible, de dépenser le moins possible et d'accumuler les réserves.

Comme vous le verrez, malgré les ressources actuelles très faibles, rendues plus faibles encore par le fait de la chute du franc français depuis notre dernière réunion, notre situation de trésorerie s'est améliorée continuellement.

Pour la location du local, chauffage, éclairage, frais généraux, paiement du Secrétaire Général adjoint, de la dactylographe, du comptable, nous avons dépensé par mois moins de 8 dollars, soit une somme inférieure à 100 dollars pour 1927.

Nous avons pu économiser, depuis 1922, tout en subventionnant les commissions, 22,000 francs environ, sans compter l'intérêt des différentes sommes et sans compter le reliquat du crédit de 50,000 francs ouvert par moi-même (¹), soit environ 60,000 francs.

Il convient, à mon sens, de garder précieusement cette réserve et d'envisager même la création de ressources nouvelles pour pouvoir développer l'activité de notre Union : de cette façon, notre avoir ne serait pas entamé par l'impression des rapports approuvés par l'Assemblée Générale de Washington et des procès-verbaux des séances de notre réunion.

Liste des documents mis en circulation par le Secrétariat Général.

Le Secrétariat Général a mis en circulation 150 documents numérotés. On peut les grouper comme suit :

I. — *Caractéristiques et résultats des mesures des émissions U. R. S. I.*

a) Du Comité national français :

Numéros 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 29, 31, 32, 33, 39, 40, 41, 43, 44, 46, 47, 50, 54, 55, 58, 62, 63, 64, 66, 68, 72, 74, 77, 78, 80, 82, 83, 85, 86, 91, 100, 105, 111, 119, 132, 134, 139, 143, 147, 150.

b) Du Comité national des Etats-Unis :

Numéros 22, 23, 24, 25, 26, 27, 28, 34, 35, 36, 37, 38, 45, 52, 56, 59, 67, 69, 75, 81, 84, 87, 99, 107, 118, 126, 127, 130, 138, 146.

c) Du Comité national anglais : Nos 46, 47.

d) Du Comité national italien : Nos 71, 73, 104, 137.

e) Du Comité national norvégien : No 49.

II. — *Orage magnétique du 26 janvier 1926.*

Du Comité national anglais : No 98.

Du Comité national japonais : No 120.

Du Comité national australien : No 125.

III. — *Relations entre les phénomènes solaires et terrestres.*

No 113 : Premier rapport de la Commission instituée pour l'étude des relations entre les phénomènes solaires et terrestres.

IV. — *Opération internationale de détermination des longitudes organisée par les « Unions internationales d'Astronomie et de Géodésie ».*

Du Comité national anglais :

No 129 : « Notes on the reception and registration of Wireless time signals sent from Bordeaux and Annapolis from 1926 November 2-30 ».

V. — *Rapports des Comités nationaux aux Commissions de l'U. R. S. I.*

Du Comité national anglais :

A la première commission : No 131.

A la deuxième commission : Nos 93, 140, 141.

A la troisième commission : Nos 88, 142.

VI. — *Autres communications.*

Du Comité national américain :

No 30 : Radio Observations during the Solar Eclipse of September 10, 1923, by L. W. Austin.

Du Comité national anglais :

No 42 : Report on Directional Observations carried out in High Northern latitudes for the information of the Central Administration of the U. R. S. I., by R. L. Smith-Rose, Ph. D., M. Sc. (National Physycal Laboratory).

Du Comité national français :

No 51 : Les influences météorologiques sur la propagation des ondes par le Capitaine Bureau de l'Office national Météorologique.

Du Comité national américain :

No 53 : A suggestion for Experiments on Apparent Radio Direction Variations, by L. W. Austin.

No 60 : Some transpacific Radio Field Intensity Measurements, by L. W. Austin.

No 61 : Field intensity curves for various distances and frequencies by the Austin-Cohen Formula.

Du Comité national français :

No 61*bis* : Onze mois d'observations des atmosphériques.

Les atmosphériques. Leur classification. Leurs propriétés thermodynamiques, par le Capitaine Bureau, de l'Office national Météorologique.

Du Comité national américain :

No 65 : Observations during the Solar Eclipse of January 24, 1925, by L. W. Austin.

No 70 : Long distance Radio Receiving Measurements in 1924, by L. W. Austin.

Du Comité national français :

No 79 : Les atmosphériques sur les Océans. Leurs caractères météorologiques, par MM. Bureau et Goyccque.

Du Comité national anglais :

No 89 : On some Direct Evidence for Downward Atmospheric Reflection of Electric Rays, by E. V. Appleton, M. A., D. Sc., and M. A. F. Barnette, M. Sc.

No 94 : An investigation of Wireless Waves arriving from the upper Atmosphere, by R. L. Smith-Rose and R. H. Barfield.

No 95 : An instantaneous direct-reading Radiogoniometer, by R. A. Watson Watt, B. Sc. (Eng) and J. F. Herd.

No 96 : The directional Recording of Atmospherics, by R. A. Watson Watt, B. Sc. (Eng). .

No 97 : The propagation of Radio Waves, by J. Hollingworth, M. A., B. Sc.

(¹) Ce crédit avait pour objet principal de faciliter les travaux de début de l'U. R. S. I., avant l'adhésion des différentes nations; il a été fait rapport à l'Assemblée Générale de 1922 sur les dépenses effectuées, rapport qui faisait ressortir le reliquat à 36,677 francs.

Du Comité national américain :

No 101 : Direction Determinations of Atmospherics Disturbances on the Isthmus of Panama, by L. W. Austin.

Du Comité national italien :

No 102 : Metodo per osservare gli atmosferici della Radiotelegrafia, E primu resultati ottenuti, par D. Bernardo Paoloni O. S. B.

Du Comité national japonais :

No 103 : Experimental Determination of Fundamental Dynamic Characteristics of a triode, by E. Takagishi.

No 106 : Projector of the Sharpest Beam of Electric Waves, by Hidetsugu Yagi and Shintaro Uda.

Du Comité national anglais :

No 108 : Discussion on the Electrical State of the Upper Atmosphere opened by Sir Ernest Rutherford, O. M.; F. R. S.

No 109 : On lightning, by G. C. Simpson, F. R. S.

No 110 : The cause and elimination of night errore in Radiodirection finding, by R. L. Smith-Rose, Ph. D. M. Sc. and P. H. Bartfield, M. Sc.

No 112 : Variations of apparent bearings of radio transmitting stations.

Part. III. — Observations on ship and shore transmitting stations, November 1922; March 1924, by R. L. Smith-Rose.

Du Conseil international de Recherches :

No 113 : Premier rapport de la Commission instituée pour poursuivre l'étude des relations entre les phénomènes solaires et terrestres.

Du Comité national américain :

No 114 : List of Scientific Publications of the Laboratory for Special Radio Transmission Research formerly the U. S. Naval Radio Research Laboratory, by L. W. Austin.

No 115 : Papers presented at Annual Meeting of the American section of U. R. S. I., April 24, 1926.

Committee 1. — Methods of Measurements and Standards.

General Report on Progress of Radio Measurements, by J. H. Dellinger.

Notes on the Measurements of Radio-Frequency Resistance, by A. Hund.

Symposium on Piezo Oscillators (No record made), by W. G. Cady, A. Hund, A. H. Taylor.

Committee 2. — Radio Wave Transmission Phenomena.

General Report on Work of the Committee, by L. W. Austin.

Cooperative Measurements on Radio Fading in 1925, by J. H. Dellinger, C. B. Jolliffe, T. Parkinson.

Committee 2a. — Variations of Radio Wave Direction. Properties of Waves Refracted, by the conducting Layer, by G. Breit.

A new Direction Finder, by F. W. Dunmore.

Committee 2b. — Wave phenomena above 3 megacycles.

Relation between the height of the Kennelly-Heaviside Layer and High-Frequency Radio Transmission Phenomena, by A. H. Taylor.

Hertz antenna reception and Radio Direction errors (Included in article « The Kennelly-Heaviside Layer and Radio Wave Propagation » in Jour. of the Franklin Institute, 201, p. 597, May 1926), by E. O. Hulburt.

Fading Observed at Washington in special WGY Transmissions, by T. Parkinson.

Polarization Changes caused by Ground Absorption, by E. F. W. Alexanderson.

Committee 3. — Atmospheric Disturbances.

General Report on the Study of Atmospheric Disturbances, by H. T. Friis.

A static recorder (Published in the Bell System Technical Journal, 5, p. 282, April 1926), by H. T. Friis.

Attenuation and frequency distribution of Atmospherics, by C. N. Anderson.

The direction of Atmospheric Disturbances in Panama, by L. W. Austin.

Du Comité national japonais :

No 116 : On the Accuracy of Wireless Time Signals, by Masao Hasimoto. Tokyo Astronomical Observatory.

Du Comité national anglais :

No 117 : On the nature of Atmospherics. Parts II and III, by E. V. Appleton, R. A. Watson and J. F. Herd.

No 121 : The Piezo-electric Quartz Resonator and its equivalent Electrical Circuit, by D. W. Dye, B. Sc.

Du Comité national japonais :

No 122 : Short Wave Communication Test Across the Pacific, by Toyokichi Nakagami.

No 123 : Receiving Test of Short Wave Radiosignals from Nauen, by Toyokichi Nakagami.

Du Comité national français :

No 124 : Les Atmosphériques sur les Océans. Etude d'observations faites sur l'Atlantique Nord de novembre 1924 à juin 1925, par R. Bureau et M. Goyecque.

Du Comité national anglais :

No 128 : On Wireless Interference Phenomena between Ground waves and waves deviated, by the upper Atmosphere, by E. V. Appleton and M. A. F. Barnett.

Du Comité national japonais :

No 133 : On the Transit Velocity of Radio Wave along Geodesic Line, by Masao Hasimoto.

No 135 : Propagation of short Radio Waves, by Hantaro Nagaoka, M. I. A. (Institute of Physical and Chemical Research.)

No 136 : Tables of Self-Inductance of Circular Coils and of Mutual Inductance or Coaxial Circular Currents, by Hantaro Nagaoka and Sadazo Sakurai. (Institute of Physical and Chemical Research.)

No 144 : On clicks and Grinders of Atmospherics, by Hantaro Nagaoka, M. I. A.

Du Comité national américain :

No 145 : Preliminary Observations on Solar Activity and Radio Reception. Laboratory for Special Radio Transmission Research. Bureau of Standards.

Du Comité national anglais :

No 148 : On the Nature of Wireless Signal Variations, Parts I and II, by E. V. Appleton, F. R. S. and J. A. Ratcliffe.

No 149 : Short Wave Wireless Telegraphy, by T. L. Eckersley.

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Du Comité français :

Rapport sur les observations des signaux U. R. S. I., effectués à Meudon, par René Mesny.

Rapport sur les différentes méthodes utilisées dans les mesures de rayonnement des signaux U. R. S. I., par René Mesny.

Résultat des mesures des signaux U. R. S. I., effectués à l'Institut de Géophysique de Strasbourg.

Projet de programme de l'opération préliminaire de longitude que l'on se propose d'effectuer à la fin de 1923.

Deuxième série des essais systématiques d'émission de la station transcontinentale de Sainte-Assise.

Du Comité américain :

A decimal classification of Radio Subjects. An extension of the Dewey System. Department of Commerce. Circular of the Bureau of Standards, n° 138, March 1923.

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Long Distance Radio Receiving Measurements at the Bureau Standards in 1923, by L. W. Austin.

Our present knowledge concerning the Atmospheric Disturbances of Radio Telegraphy, by L. W. Austin, 1924.

A Method of Measuring Radio Field Intensities and Atmospheric Disturbances, by L. W. Austin and E. B. Judson, 1924.

Field intensity Measurements in Washington on the Radio Corporation Stations at New Brunswick and Tuckerton, N. J., by L. W. Austin, 1924.

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A portable Radio Direction Finder for 90 to 7,700 kilocycles, by F. W. Dunmore. Scientific Papers of the Bureau of Standards, n° 536, October 1926.

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On a Standard of Mutual Inductance, by A. Campbell, B. A.

On the Measurements of Alternating Electric Currents of High Frequency, by A. Campbell, B. A. and D. W. Dye, B. Sc.

The Effect of Underground Metalwork on Radio Direction Finders, by R. L. Smith-Rose, M. Sc. and R. H. Barfield, B. Sc.

Electric Oscillations in Straight Wires and Solenoids, by J. S. Townsend, M. A. and J. H. Morrel, M. A.

Capacity and Eddy Current Effects in Inductometers, by S. Butterworth, M. Sc. of the National Physical Laboratory.

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Directional Observations of Atmospheric Disturbances 1920-1921, by R. A. Watson Watt, B. Sc.

The Measurement of the Electric Intensity of Received Radio Signals, by J. Hollingworth, M. A., B. Sc.

Measurement of Signal Strength, by J. Hollingworth, M. A., B. Sc.

The Effect of local conditions on Radio Direction-Finding Installations, by R. L. Smith-Rose, M. Sc. and R. H. Barfield, B. Sc.

On the Nature of Atmospherics, I, by R. A. W. Watt, B. Sc. and E. V. Appleton, M. A., D. Sc.

The Valve-maintained Tuning-Fork as a Precision Time-Standard, by D. W. Dye, B. Sc.

Report on Standards and Methods of Measurements at Radio Frequencies, by D. W. Dye. — Commission 1.

Report on Directional Wireless Experiments made

under the auspices of the Radio Research Board, by F. E. Smith. — Commission 2.

Report of work being done by amateurs in Great Britain in connection with the « Commission de Liaison avec les opérateurs, praticiens et amateurs », by A. A. Campbell Swinton. — Commission 4.

Du Comité italien :

Bi-mensuellement la brochure « La Meteorologia Pratica », publiée par l'Observatoire de Géophysique de Montecassino, Directeur M. D. Bernardo M. Paoloni, Ben. Cass.

General Session, October 28, 1927

Général Ferrié. — This meeting will be devoted to some matters of bussiness and to the reports of the Chairmen of the four Commissions. We shall first consider matters of an administrative character upon which propositions have been formulated by the administrative committee appointed at the opening session. The recommendations of that Committee will now be submitted for your discussion and action. The first recommendation is a follows :

« 1. Il est décidé, en principe, de tenir chaque année une Assemblée générale de l'U. R. S. I. et de publier aussitôt après les travaux présentés et les discussions scientifiques dont ils ont été l'objet. En 1928, la réunion aura lieu à Bruxelles, le mois de septembre.

Les travaux présentés pourront aussi être publiés par leurs auteurs là où ils le jugeront convenable.

Les savants qui désireront présenter des travaux ou des propositions à cette Assemblée seront priés de vouloir bien les envoyer, si cela leur est possible, avant la réunion afin de permettre de les traduire, de les imprimer, de les distribuer aux Comités nationaux et de donner ainsi la possibilité de préparer les discussions, en hâtant également la publication ultérieure du compte rendu de l'Assemblée générale de 1928. »

Has anyone any remarks to make on the subject of this proposal?

Prof. Yokoyama. — I propose that a fixed time be set for the sending in of advance documents. For example, the people who may wish to present papers to the meeting might be requested to send them so as to reach the General Secretary at least three months before the meeting.

Général Ferrié. — That limit may be a little short, because if you consider a paper sent from Japan, it takes quite a time to come, and would suggest four months as the limit. Papers should be submitted and in the hands of the General Secretary at least four months before the date of assembly.

General Squier. — I suggest, Mr. President, that the words « if possible » be inserted because it might be that some very valuable piece of work would be left out.

Dr. Austin. — I would like to inquire if this applies to the papers which are to be given in the way that our papers in our public meetings this year were given?

Général Ferrié. — Yes.

Dr. Austin. — This requirement might frequently stand in the way of the latest developments of the science being presented. We could sav something like this : the authors who expect their papers to be printed and circulated must send them at least four months before the date of meeting. Those papers sent after that time could no be printed and circulated.

Général Ferrié. — It is decided that N° 1 is adopted with the modification that all papers shall, if possible, be sent to the General Secretary at least four months before the annual meeting. Dr. Goldschmidt and Dr. Le Corbeiller are appointed to revise the text of this and all the other proposals considered today, to make the wording agree with the decisions of the meeting.

« 2. Les travaux examinés et discutés par chaque Assemblée générale, ainsi que ceux qui auront déjà été distribués dans le courant de l'année qui la précède et que le Bureau de l'Union estimera devoir être joints aux précédents, seront réunis en un ouvrage rédigé à la fois en langue française et en langue anglaise. »

N° 2 was adopted.

Général Ferrié. — « 3. Un traité sera passé, si cela est possible, avec le Bureau international télégraphique de Berne pour que son journal publie mensuellement, sans frais pour l'Union, une bibliographie des travaux et volumes relatifs à la radiotélégraphie scientifique.

Chaque comité national enverra, tous les mois, au Secrétaire générale, la bibliographie des travaux de ce genre publiés dans son pays et signalera également les travaux intéressants dont il connaîtrait la publication dans les pays n'appartenant pas à l'Union. Le Secrétaire général s'efforcera de compléter autant que possible cette dernière partie de la bibliographie.

Pour chacun des travaux il ne sera indiqué que le titre du travail, le nom de l'auteur, et les indications bibliographiques nécessaires, ainsi que quelques lignes donnant une idée générale de son contenu.

Ce service commencera à fonctionner à partir de janvier 1928.

Il sera procédé à un tirage à part de la bibliographie publiée dans chaque numéro du *Journal de Berne* et chaque Comité national recevra un certain nombre d'exemplaires d'après un accord particulier entre le Secrétaire général et lui ».

Those National Comittees which require more copies of the bibliography than normal will have to make a special arrangement with the General Secretary and compensate the Union for the cost involved.

The matter of a numerical classification of subjects cannot very well be discussed here in detail and perhaps we might leave it to the General Secretary to study the matter and communicate his recommendations to the various national committees. We may use, to begin with, the classification established by the Bureau of Standards.

Dr. Goldschmidt. — I have considered suggestions on classification by Dr. Dellinger and Dr. Austin and I think I can work out a system that will embode the advantages of all hitherto used.

Général Ferrié. — Paragraphe n° 3 is adopted. Paragraph n° 4 reads :

« 4. Les comptes-rendus de l'Assemblée générale de 1927 seront publiés dans les conditions suivantes, en français et en anglais.

Le Secrétaire du Comité américain enverra à chacune des personnes ayant pris part aux discussions dans les séances de commissions, une copie du relevé de la sténographie de ces discussions afin qu'elle puisse y apporter les corrections nécessaires.

Après le retour des épreuves corrigées, le Secrétaire du Comité américain établira un texte définitif de ces discussions qu'il enverra au Secrétaire général de l'Union, en même temps que les textes des travaux présentés aux commissions et en séances publiques. Ceux de ces travaux qui ont déjà été publiés feront l'objet de résumés ètablis par les auteurs eux-mêmes, avec indication de l'endroit où à été faite la publication intégrale, si une reproduction de celle-ci présente des difficultés.

Le Secrétaire général fera imprimer tous ces textes dans leur langue d'origine et enverra de nouveau aux auteurs des épreuves à corriger. Il fera effectuer en même temps leur traduction et l'impression de celle-ci, de manière à gagner du temps et à permettre une publication aussi rapide que possible de l'ensemble du compte-rendu de l'Assemblée générale de 1927. »

Dr. Dellinger. — I will do everything possible to expedite the matter. Upon sending the material for correction by the various authors I will set a date. I would like to have the approval of the body here for setting a date beyond which if the paper is not returned I shall use the original text.

Général Ferrié. — Dr. Dellinger could fix this date himself in the letter which he would send to the authors.

Dr. Dellinger. — We greatly regret that it has been impossible to get the minutes out in time for perusal before any of the members left Washington. It was physically impossible to do this more speedily.

Général Ferrié. — Paragraph n° 4 is adopted. — Paragraph n° 5 is :

« 5. Les dépenses supplémentaires à prévoir pour l'année prochaine sont les suivantes :

a) Impression du compte-rendu de l'Assemblée Générale de 1928, en français et en anglais;

b) Relèvement de l'indemnité du collaborateur actuel du Secrétaire général, qui prendra à l'avenir le titre officiel de Secrétaire général adjoint et qui continuera à seconder le Secrétaire

général pour les questions administratives et financières ;

c) Attribution d'une indemnité à un agent scientifique qui donnera son concours au Secrétariat général pour la préparation de la bibliographie mensuelle et pour les diverses besognes ayant un caractère scientifique.

Le Secrétaire général fixera lui-même le montant des indemnités stipulées aux paragraphes *b* et *c*, ci-dessus, dans la limite des ressources disponibles. »

Paragraph n° 5 was adopted.

Général Ferrié. — « 6. Les Comités nationaux feront des démarches auprès de leurs Gouvernements et Académies, des Sociétés commerciales, des groupements d'amateurs et des particuliers s'intéressant à la télégraphie sans fil, dans le but de recueillir soit des dons soit des cotisations annuelles, soit des souscriptions aux publications de l'Union. Une circulaire signée par le Président et le Secrétaire général de l'Union sera adressée à cet effet par ce dernier aux Comités nationaux, qui les joindront aux lettres qu'ils enverront dans ce but.

Une lettre sera également adressée par le Bureau de l'Union à tous les Présidents des Délégations à la Conférence internationale radiotélégraphique de Washington actuellement réunie, pour leur demander de vouloir bien faire tout ce qui dépendra d'eux pour obtenir l'adhésion de leur Pays à l'Union internationale de radiotélégraphie scientifique. »

Dr. Van der Pol. — It would be well if we had a rough idea of what would be the price of the volume of papers of the 1927 meeting, for which subscriptions are to be solicited.

Général Ferrié. — Dr. Goldschmidt might perhaps be able to tell us what the price would be.

Dr. Goldschmidt. — We might give a price, saying it would only be tentative. The suggested price is 100 francs or $ 4.

Dr. Dellinger. — If I understand correctly, this letter which is to be sent will presumably ask for donations for supporting the Union and also ask for subscriptions to this book. Now it occurs to me that it would be better to handle the two things separately. There are many organizations which have heard very little about this Union, and it will be a little confusing to them. We could probably arouse more interest if we would first send a letter describing the book very completely. Then after the book has been distributed we could send a request for subscriptions, and having become familiar with the Union through seeing this book, they might be more interested and subscribe more generously.

Général Ferrié. — Perhaps the radio companies and the governments, who have more considerable amounts at their disposal than individuals, are more familiar with the U. R. S. I. and could be asked at the same time in the same letter to send both donations and subscriptions to the book, whereas the remarks of Dr. Dellinger apply rather to individuals, who would first have to be told about the work we are doing. It is important that the request for funds should not be unduly delayed. Anyway, we could leave it to each national committee of handle the matter to the best of their judgment.

Dr. Dellinger. — My main point is to separate the two things. I agree entirely with the idea that the request for subscriptions should not be delayed, and quite likely it would be very desirable, here and in some other countries, to send letters requesting donations immediately and leave the matter concerning the book to a later time. The two things are liable to, become confused if the two requests are sent out at the same time. The book is a matter of only $ 4 and we hope in some cases to secure subscriptions more like $ 400 or $ 4,000, so we do not want to confuse them.

Général Ferrié. — The reasoning is good, and a separation of the two requests is desirable.

Dr. Van der Pol. — It will be very important to prepare very carefully the list of people to whom these letters are going to be sent. I suggest that a draft of these letters be prepared by our General Secretary. Perhaps we can give today several addresses of promising prospects.

Dr. Goldschmidt. — The list of adresses in each country should be left to the National Committees. The National Committees might find it desirable to have special subcommittees for the purpose, including someone in the country who plays a prominent part in radio such as Mr. Hoover in the United States.

Général Ferrié. — We all agree to allow the greatest freedom to the national committees, who are in a better position to appreciate the local circumstances.

Paragraph n° 6 is adopted.

Paragraph n° 7 reads : « 7. Il est décidé en principe d'attribuer des prix, médailles ou diplômes internationaux pour récompenser les auteurs de travaux importants ou les personnes ayant fait preuve d'un grand dévouement pour aider les travaux scientifiques de l'Union. Toutefois, cette question ne sera étudiée en détail qu'à l'Assemblée générale de 1928. »

N° 7 was adopted.

« 8. La création de Membres ou de Président d'honneur de l'Union sera également examinée par l'Assemblée générale de 1928. »

N° 8 was adopted.

« 9. L'Assemblée générale de 1927 reconnaît la nécessité d'essayer de développer progressivement, et au fur et à mesure de l'accroissement des ressources de l'Union, l'organisation du service de la bibliographie et des publications, et de constituer avec l'aide des Comités nationaux, une bibliothèque centrale de Radiotélégraphie scientifique. Le but à atteindre est de pouvoir mettre à la disposition des savants de tous les pays adhérents à l'Union, toutes les publications qui leur seraient utiles.

Des démarches seront faites par le Secrétaire général, pour commencer, auprès de tous les éditeurs de revues, ouvrages et publications diverses se rapportant à la radiotélégraphie scientifique pour obtenir l'envoi gratuit, à l'Union, de deux exemplaires au moins de chaque publication. »

N° 9 was adopted.

« 10. La cotisation annuelle des Etats adhérant à l'Union sera dans l'avenir de 100 francs-or par unité, au minimum. Les Comités nationaux s'efforceront, par tous les moyens à leur disposition, d'augmenter autant que possible les versements faits à l'Union. »

Dr. Dellinger. — On this particular subject I think it would be well to give not only this conclusion but also an explanation to be prepared by the General Secretary as to the amount formerly paid and the units of payment as stipulated in the Constitution, etc.

Général Ferrié. — N° 10 is adopted, with the understanding that the General Secretary will prepare an explanatory statement to be added. Paragraph n° 11 is :

« 11. Le renouvellement du Bureau de l'Union sera fait à la fin de l'Assemblée générale de 1928. Les Présidents des Commissions scientifiques seront à l'avenir les Vice-Présidents de l'Union, dont le nombre sera porté à 4.

Tous les Membres du Bureau de l'Union devront appartenir à des Nations différentes. »

N° 12 is closely related and we shall consider them together.

« 12. Il est créé un Comité exécutif comprenant les membres du Bureau de l'Union ainsi qu'un délégué de chaque nation appartenant à l'Union.

Ces délégués seront désignés par les Comités nationaux eux-mêmes.

Le Comité exécutif sera chargé de l'étude de toutes les questions relatives à l'organisation et à l'administration de l'Union. Il pourra être consulté éventuellement par correspondance. »

I asked at the opening session to be replaced as President of the Union but considering more in detail the statutes of the Union we find that the Bureau consists of a President, Vice Presidents, and General Secretary, elected by the General Assembly which remain in office until the end of the second General Assembly following the date of the election. So according to the statutes these officers should remain in office until 1928.

Prof. Yokoyama. — If N° 11 should be adopted, in anticipation that the number of Commissions may be increased in the future it seems better that all the chairmen of the Commissions should be Vice Presidents, rather than limiting the number to four.

Dr. Dye. — It might so happen that the best person to be on the Executive Committee from a particular country might not be the person who happened to be chairman of one of the Commissions.

Général Ferrié. — That is true, and the idea was that each country should be represented by only one person, but I have no objection to changing this. However, if there were two persons belonging to the same country on the Executive Committee they might be of different opinion and it would complicate matters.

Dr. Austin. — A formal proposal has been submitted by the American Committee which contemplates the election of one person from each National Committee to be on the Executive Committee and the election from these of the officers of the Union.

That would eliminate the possibility of there being more than one representative from each country. This proposal is strengthened by Dr. Dye's point, in other words, the same person might not be qualified to be a President of a Commission and a member of the Executive Commitee.

Dr. Dye. — It seems to me it should be left entirely to the National Committees to elect their own member irrespective of the Presidency of the Commissions.

Dr. Austin. — The American proposal does not contemplate that the heads of the Commissions are necessarily to be appointed in this way or that. We did not consider that question. When there were only a few countries in the Union then all the international representatives were members of the Bureau, but of course if we are not to have the number of vice presidents coincident with the number of representatives in the different National Committees, which seems wise, then we would have an Executive Committee and the Pesident, Vice President and Secretary. The American idea, which is supported by the experience of some of the other Unions, is that the officers on the Executive Committee should be chosen by the National Committees rather than by the General Assembly. There has been dissatisfaction in some of the other Unions because the General Assembly has chosen as one of the officers of the Union a person who would not be pleasing to his National Committee.

Général Ferrié. — I would like to know how you would suggest that the chairmen of the Commissions be chosen.

Dr. Austin. — The American Sections's proposal did not consider that question. I suppose that the chairmen of the Commissions should be chosen by the General Assembly. This would not contemplate having them ex-officio vice-presidents.

Général Ferrié. — It is decided that n° 11 and n° 12 are to be modified to provide that each nation will name a representative on the Executive Committee. The General Assembly will choose the president and two vice-presidents from among the members of the Executive Committee. The chairmen of the Commissions will be chosen by the General Assembly; they shall not necessarily be members of the Executive Committee. The Bureau of the Union will consist of the president, two vice-presidents and the general secretary.

N° 13 reads :

« 13. La Commission des mesures pourra s'occuper éventuellement de l'étude de certains appareils, au point de vue scientifique seulement, et prendra le nom de Commission des mesures et appareils.

La Commission de liaison devra s'occuper à l'avenir des questions concernant la liaison avec les expérimentateurs volontaires ou amateurs, mais aussi de celles qui lient la radio-télégraphie à d'autres sciences telles que la géophysique, l'astronomie, etc. »

Dr. Van der Pol. — I am very pleased to hear this last paragraph and I may say that we expect much more from cooperation with the geophysicists and astronomers tham from cooperation with amateurs only. I would like to state another point in connection with the first paragraph. I have noticed in these meetings, and in the papers we read, that there are several scientific fields which are not covered and wich I think it is difficult to put in the Commission on Measurements and Apparatus. I am thinking of the work of Carson and the work of Kennelly, the work of Le Corbeiller, and that of General Squier. I refer to questions such as the theory of triodes and the general theory of circuits. I expect some important developments in the near future in connection with the general theory of complex functions, also further experiments and theory of telephone receivers and loud speakers, and questions of modulation. I dot not see any reason to put them under the heading of measurements and apparatus, and I would like to ask the opinion of those present as to having a separate Commission consider these points, I do not suggest a name for this Commission; perhaps we could find some collective name for all these important parts of radio science.

Dr. Le Corbeiller. — I wish tot support the proposition of Dr. Van der Pol.

Dr. Kennelly. — I think that the remark made by Dr. Van der Pol is correct in regard to the desirability of considering certain subjects that do not fall well within the scope of the first Commission, that on measurements and apparatus. They, of course, might be put into that Commission, but the name does not suggest those other subjects. It is a little difficult to find a name for the proposed new Commission, which refers to radio circuit theory in general; but it would seem that there is a large field for that kind of work in the future as the science of radio grows; and it is growing very fast. I therefore desire to support the proposition that has been put forward by Dr. Van der Pol and already seconded by Dr. Le Corbeiller.

Dr. Goldschmidt. — I think our Commissions should include all that is of interest in the art of radio. Would it not be proper to call this the Commission dealing with everything that is not dealt with in other Commissions?

Dr. Lecorbeiller. — I think it is indeed difficult to find a name for this Commission but I do not think it would be the least advantage of this Commission if it should help to attract interest to this new group of questions, which have not yet been included in the matters taught in the universities.

Dr. Vanni. — I am completely in accord with the suggestion made by Dr. Van der Pol and supported by other members. I think there are numerous questions that might be taken up in this Commission. For instance, I do not feel that a completely satisfactory theory of the telephone has been found, although it has been in use for a very long time. That kind of study might very properly fall within the scope of such a Commission.

Dr. Van der Pol. — I may add that I heard recently that in one of the universities in Germany a chair was founded which I think was called « General Theory of Oscillations », so Germany is aware of the importance of theoretical and experimental development of oscillations. Perhaps this may be an indication, moreover, that the theory not only of electrical oscillations, but mechanical oscillations as well, will be important in the future science of radio.

Dr. Lecorbeiller. — I recall that at least fifteen years ago there was for a few years in the University of Paris a chair on the general theory of oscillations which was confided to Guillet who is still working at the Laboratories; he published a book which at the time was an extremely new venture.

Général Ferrié. — Would anyone else like to give an opinion on the proposed creation of this Commission? We could settle later on the name. Dr. Austin?

Dr. Austin. — There seems to be plenty of reason why this Commission should be organized.

Prof. Yokoyama. — I have no opinion on it.

Dr. Dye. — I am of the opinion that it would be very valuable to have auch a Commission.

Général Ferrié. — We are all in accord with the idea of having a fifth Commission. I suggest that we give it some provisional name with the understanding that when we meet again next year we may change the name if we wish. The Commission on Oscillations will be the provisional name.

Dr. Austin. — I would like to ask if it is the idea to have the new Commission cover the theory of wave propagation that some of these gentlemen are working so vigorously on.

Général Ferrié. — The understanding is that there is not to be any duplication. Any question not coming in the scope of the other four Commissions and relating to the general study of oscillations, will be handled by the new commission.

No. 13 is adopted, and there is to be added thereto a provision that a fifth Commission is to be created wich will have for its field the general theory of oscillations.

No. 14 reads : « 14. Le vote par correspondance sera admis pour toutes les questions soumises à l'avance aux Assemblées générales auxquelles certaines nations adhérentes ne pourraient pas se faire représenter. »

N° 14 is adopted.

N° 15 reads. — « 15. Le Bureau de l'Union est chargé d'apporter au texte des statuts les modifications qui résultent de l'adoption des mesures indiquées dans les paragraphes qui précèdent. Le texte définitif sera soumis à l'approbation de l'Assemblée générale de 1928. »

N° 15 is adopted. Has anyone anything else to propose?

Dr. Hulburt. — I should like to express the hope that the name be changed. I hope it will be possible soon to drop the name « scientific » in the name of the U. R. S. I.; that the work be sufficiently well known as not to require that the word « scientific » being mentioned.

Général Ferrié. — I think our Union is still a little young to drop the word « scientific ». Many nations are not members and it is desirable to make the aim of our work perfectly plain. In the future it may be desirable to drop the word.

Dr. Austin. — The real difficulty, as I understand it, is that the French language can not use the word « radio » as an adjective.

Général Squier. — There seens to me a more serious objection. « Radio Telegraphy » is in the name, and that is of course out of date. It might be well at the next conference to consider changing telegraphy to communication or something of the sort.

Dr. Goldschmidt. — It would be dangerous to change the name. The U. R. S. I. is beginning to be very widely known under that name.

Dr. Lecorbeiller. — The name could be changed without the initials being changed.

Général Ferrié. — It is agreed that the following paragraph be added :

« 16. — The General Assembly of 1928 shall consider the change of the name of the Union, in view for instance of the restrictive sense of the word « telegraphy ». »

There are a few further items of business. The accounts presented by the General Secretary in the opening session have been approved and therefore do not have to be considered anew today. Also the question of the agenda of our next meeting has already been considered.

The British Committee has asked us to publish a list of the short-wave stations and keep this list up to date. It is understood that the General Secretary will do so.

There was also a suggestion by Dr Austin concerning the correlation of radio work with solar studies, but we might consider that as coming under the work of the Commission on Liaison.

I shall now ask the Presidents of each Commission to submit reports on the work of their Commissions.

The reports follow herewith.

Report of Commission I.

The writer was appointed acting chairman of Commission I at the first General Assembly.

Since no general report embodying the outlines of the activities of Commission I, as collected from the various national reports, had been prepared by the President of the Commission, a general statement amounting to such a report was put together by the chairman and presented in sections to correspond more or less with the matters set out for discussion in the General Secretary's letter of August 5.

The discussions whilst following roughly along these lines were actually in the nature of an interchange of ideas and experiences rather than of means of attaining specified objects.

This interchange was of much interest and value quite independently of any conclusions drawn from the discussions.

The first meeting held on October 12 was confined entirely to considerations of frequency, its standardization and dissemination.

A very interesting discussion followed the introductory remarks of the chairman who outlined the general trend of the evolution of accurate frequency measurement in the various countries.

This discussion was recorded verbatim and is given in full in an accompanying report, since the remarks were all of interest at this meeting it was felt that they should be preserved in this manner.

It will be seen that harmonic methods of stepping upwards and downwards in frequency are the only ones which have proved themselves equal to the severe requirements of the present time. It may be said that any one of a number of such methods is now available to form a satisfactory basis for the realisation of a standard of frequency based directly upon the second as a unit of time. It was universally agreed that the unit of frequency must be directly related thereto. The accuracy desired was considered to be not less than one part in a hundred thousand, an accuracy which is now within reach.

The discussions further dealt with the use of piezoelectric quartz oscillators as frequency standards and as one convenient means whereby the international standards of different countries and the standards used by various bodies in one country may be compared. It was generally agreed that carefully constructed piezo oscillators form one of the best secondary standards when operated under constant battery and circuit conditions and provided with temperature control.

It was seen from the discussions that considerable theoretical and experimental work on piezo crystals was in progress and that the results of this work, together with a critical resume which the writer has in preparation collecting the information available up to the present time, would advance our knowledge of this important subject.

The possibilities of using waves transmitted from certain carefully selected stations were also discussed.

There are stations which are probably sufficiently steady to serve the purpose of intercomparing standards both in Europe and America. In particular, at the Naval station at Arlington, short waves are available which are generated, using temperature controlled piezo master oscillators to control the frequency. The transmissions are made on four frequencies of approximately 4, 8, 12 and 16 megacycles per second. The frequencies are all exactly harmonic to one another. Time signals are sent out twice a day on all the four waves practically simultaneously.

Général Ferrié pointed out that such waves could be used to observe changes in the apparent velocity with change in frequency consequent upon the various paths selected by the various waves of different frequencies. The observations would consist in recording the time signal on each wave on a particular day at a number of suitably disposed stations on widely separated parts of the earth. The intervals separating the signals of different frequencies would be compared for the various stations and the differences would give a measure of the retardation of one wave from another.

The ordinary Morse traffic on these waves could be used for frequency measurement purposes, since the persistence of the controlling frequency provided by the steadily running piezo oscillator would carry through the Morse signals without discontinuity of phase.

The possibilities of these waves were discussed and it was considered that some preliminary measurements should be made in a few widely separated points. By simultaneous recording of local astronomical time additional information regarding latitude, etc. would be obtained.

Commander Taylor of the Naval Station offered to help in any way he could by giving information regarding the transmissions and giving particulars regarding the reception of these short waves. Special signals could also be sent if necessary.

At the second meeting, the discussion on frequency was resumed. In the meanwhile, a number of papers bearing on the subject had been presented and read at a general session appointed for the purpose. These meeting occupied two days altogether and of the twenty-four papers presented four had direct reference to frequency measurement.

The papers showed that a very considerable amount of attention had been given to the subject in Americawhere it is considered of great importance.

Standard piezo oscillators developed by the Naval Department and others developed at the Bell Telephone Laboratories were described and shown. In addition, a very careful experiment made at the Bell Laboratories was described in which comparison observations between two very specially constructed piezo oscillators of nearly identical type were made. The experiments showed that, under the carefully controlled conditions, each oscillator maintained extraordinary constancy for both short and moderately long periods of time. The results will be published shortly so that it will suffice to say that the experiments indicate that constancy to one part in a million may confidently be expected of an oscillator when sufficiently carefully mounted and accurately temperature controlled.

The possibility of using quartz resonators additionally to the oscillators for comparison purposes was raised by the chairman and a few remarks pointing out the advantages of cheapness, simplicity, ease of transport, and other qualities of resonators were made. These remarks were endorsed by Prof. Cady who was present at this meeting.

As a result of these discussions it was considered desirable to continue the circulation of piezoelectric temperature controlled oscillators to different countries as already initiated and organized by the Bureau of Standards. The chairman remarked that he thought that, with the concurrenee of the British National Committee, he could have constructed and circulated a number of resonators to serve as additional and somewhat different standards for intercomparison purposes.

One of the Japanese delegates proposed that they should use as their standard of frequency, the valve maintained tuning fork supplied to them with a certificate of its frequency, etc. as supplied by the National Physical Laboratory and then build upwards from this to piezoelectric oscillators whilst further developing their basis directly from the second. The Japanese delegate also expressed the desire to cooperate in future intercomparison measurements of the national radio frequency standards by means of the circulated piezo oscillators.

The third meeting commenced with a short statement by the chairman relative to standards and methods of measurement of inductance, capacity, effective resistance, voltage, current etc., suitable for radio frequencies. The importance of these basic quantities was pointed out and the suggestion was made that it would be advantageous to interchange such standards between different countries. The chairman mentioned that, in this connection, he had arranged with Dr. Curtis of the Bureau of Standards to carry out measurements of the capacities of a standard subdivided mica condenser belonging to the Bureau and also to send to the Bureau for measurement two standard inductance coils.

It was further remarked that at high radio frequencies the difficulties of all such measurements were very greatly increased so that it was difficult to separate true capacity and inductance in a circuit or even to define these quantities in terms of the current at some defined point in the circuit. In the discussion which followed Prof. Vanni remarked that in the usual method of measurement of the capacity of inductance coils the results showed that the capacity was a function of frequency. The chairman replied that whilst it appeared to be true that at frequencies very near to the resonant frequency of the coil the line connecting the reciprocal of N^2 with added capacity was usually somewhat curved, this could generally be attri buted to a real change in the self inductance of the coil with frequency so that it became difficult to separate the two effects. The internal inductance of condensers also introduced a variable which required an organized series of experiments on coils of calculable high frequency inductance for its determination. A fairly good basis for these measurements had been established in Great Britain both at the

National Physical Laboratory and at the Naval Signal School whereby the true values of small inductance coils and small capacities could be measured at high frequencies.

Prof. Vanni then showed a model of a condenser dəsigned to give a uniform frequency scale and referred to a paper by L. Sacco of the Italian Military dealing with the theoretical design of « Differential Condensers ».

A few copies of this paper were distributed to the members. The paper deals specially with cases where it is necessary to separate, on the scale of the condenser, station only 10 kc/s apart although each may be a few thousands of kilocycles per second.

The chairman remarked that uniform frequency condensers were now in very common use both in England and American and the principles of their theoretical design had been worked out in both countries. As far as he was aware the chairman believed he was the first to give the theoretical design in his paper on a standard wavemeter published in the Royal Society Proceedings in 1924.

With regard to current measurements at very high frequencies Prof. Mesny referred to a simple form of hot wire milli-ammeter for short waves in which no auxiliary leads going to a galvanometer or other indicating instrument were used.

The device consisted of an almost straight wire in an evacuated glass vessel. The wire was held taut by a small transverse force at its center. The observations were made directly on the middle of the wire with a microscope. The sensitivity was such that 15 scale divisions deflection were produced by a current of 1.5 milliamperes using a wire of about 50 ohms resistance.

The chairman was interested in the device as also was Dr. Austin. It was pointed out that the method was only suitable for small currents.

The chairman remarked on some experiments made by himself on a method of measuring a large current in terms of a much smaller one using condenser division of the currents. The method showed promise but required developing further using specially constructed condensers with very small internal inductance.

The chairman then referred to field intensity measurements remarking that unfortunately he had had but little experience in this branch of measurements other than that connected with the development of a transformer method of attenuation of a locally generated voltage for calibration purposes.

The types of apparatus used were very varied in type and method of use. Nearly every investigator developed his own method of calibration and very few indeed tested its validity by using a different method to check it. The actual method of observing, the type of receiving system, the frequency and the magnitude of the field intensity under measurement were all independent variables and every kind of combination might be used. The task of unifying these methods was therefore very difficult.

However, at the Bureau of Standards a certain amount of work had been done by the comparison of three types of long wave field intensity measuring apparatus suitable for fairly strong signals. The results of these measurements were briefly recorded in the American Report of Commission I. Dr. Austin made a few remarks at the meeting in support of the meeting in support of the statements made in the Report.

A representative from the Bureau of Standards also referred to the development of a standard apparatus for field intensity measurements in the broadcast band of frequencies with which, it was understood, comparative tests would be made with other forms of apparatus used in America.

Dr. Dellinger thought it would probably be in two years' time that definite information would be available.

Prof. Vanni referred to the apparatus developed at the Bell Telephone Laboratories which was considered to be of good accuracy. He asked whether particulars regarding its construction were available. It was mentioned that a description in outline had been published in the Proceedings of the Institute of Radio Engineers.

Dr. Dellinger thought that there would be no difficulty in obtaining full particulars of the apparatus which he considered to be of good accuracy. He understood also that the apparatus would be placed on the market shortly.

The chairman remarked that it would probably be helpful to have the following work carried out :

a) To intercompare various methods of obtaining small voltages used for calibration of the receiving system;

b) To prepare a critical resume of the various methods at present in use in various countries. He thought that some of the work involved under both headings might be done in England and would bring the matter before the National Committee.

In connection with the radiation intensity of a transmitting station Prof. Vanni read a short note relative to a new unit to represent this quantity.

The note was an extract from the *Bolletino Radiotelegrafico del R. Eserclio,* April 1926, n° 12 » by L. Sacco entitled « Proposta di una nuova unita di misura R. T. »

The proposal is to introduce a unit based upon a line integral of magnetic force round any closed path surrounding the antenna. The name proposed for this unit is « Forza Cimomotrice » and corresponds with its analogue « magnetomotive force. »

Expressed in electromagnetic units the quantity becomes

$$F = 3 \times 10^{10} \times 4\pi \frac{h_r \times i}{\lambda \times r}$$

This, in volts per meter distance from the antenna is given by the expression

$$F = \frac{120\pi\, h_r\, i}{\lambda r} \text{ (volts per meter)}$$

Prof. Vanni pointed out that such a unit would place upon a directly comparable basis, stations of widely differing wave lengths, effective heights and antenna currents giving numerical examples to emphasize the point.

The chairman remarked that such a unit was equivalent to expressing the power of a station in terms of the voltage per unit distance from it. It was also a very rational unit from an experimental standpoint since, in practice, the power of a transmitting station has to be measured in terms of the field intensity at points in the neighborhood. It then becomes unnecessary to convert these measurements into « meter amperes » as at present. In answer tot a question asked of Prof. Mesny regarding the measurement of received intensity on very short waves, he replied that in some experiments the calibration of the apparatus was made by the help of an oscillator of frequency sub-harmonic to that of the waves under measurement.

His experience was that the proportion of the required harmonic present in the oscillator remained fairly constant from day to day. The reason for the adoption of this method was the extreme difficulty of obtaining a suitable attenuation when a local oscillator of high frequency was used and the ease of measurement of the larger current at the lower frequency by the use of a source which is sub-harmonic. The experiments were still in progress and would be proved more fully later on.

A fourth, short meeting, was held to review the position particularly from the point of view of the items definitely set out in the General Secretary's letter of August 5 to Commission I and to make recommendations regarding any proposals received from members present at the meetings.

The following proposals were agreed to :

1. *Frequency Measurements.* — 1 *a*) To continue intercomparisons of frequency standards of the different nations by means of piezo oscillators and resonators sent from laboratory to laboratory.

For this work the experience gained from the measurements made using the oscillators already circulated by the Bureau of Standards places them in a favorable position to provide and continue the circulation of temperature controlled oscillators to this end.

The National Physical Laboratory will, with the consent of the British National Committee, provide and circulate suitable resonators to form an independent and somewhat different form of link between the laboratories.

The Japanese nation will be included in the international comparisons.

1 *b*) A few stations of great constancy in frequency may be observed in order to determine whether they are satisfactory for purposes of international comparisons.

In particular, the short wave crystal controlled transmitting stations of the Navy at Arlington, which emit four waves at frequencies harmonic to one another, between the ranges of 4,000 and 16,000 kilocycles per second, will be studied to determine their value both from the standpoint of frequency standards and for purposes connected with wave-propagation.

The experiments should, if possible, be made in Japan, United States, Europe and Australia and other countries interested.

Commander Taylor of the Naval Station, Arlington, will help in the manner already indicated.

2. *Field Intensity Measurements.* — It was realized that intercomparison of various methods presented great difficulties. Insufficient discussion was given to the subject to enable any definite course of action to emerge.

The subject was partially discussed at one of the meetings of Commission II. It is realized that the subject requires the collaboration of the members of both commissions if a definite programme of work is to be drawn up. It is anticipated that at the next general meeting at Brussels the subject will be in more definite shape for discussion.

The question of the investigation of interference from transmitting stations both for telegraphy and telephony was on the agenda for discussion but in the General Secretary's letter of August 5 it was stated that this question was thought to be outside the scope of the U. R. S. I. No very great interest appears to be taken in the subject; it was therefore not discussed.

The matter is receiving some attention at the National Physical Laboratory under the auspices of the Radio Research Board both on the theoretical and on the experimental sides; from these investigations it is probable that the subject will be reduced to a condition such that interference can be properly defined and measured. The matter can then be discussed more accurately at the forthcoming reunion.

(*Signed*) D. Dye,
Chairman.

Report of Commission II.

A considerable portion of the time of Commission II was given to the theory of wave propagation in the upper atmosphere, the proof of the existence of the Kennelly-Heaviside layer and its probable nature and position. No definite new conclusions were drawn but within fairly wide limits there seems to be a general agreement as to the height of this layer and as to the probable general theory of the bending of the radio waves. Professors Appleton, Van der Pol, Taylor, Hulburt, and Breit took prominent part in these discussions, all of whom have been very active in the experimental work which is at the base of our ideas on the transmission of the waves through the upper atmosphere.

Among the points brought out was the evidence of the existence of more than one reflecting layer, which is one of the most interesting of the more recent conclusions. Another important question discussed was that of the so-called skip distance of short waves. This has been observed almost everywhere in the world and yet there seems to be a great deal of evidence that in the tropical regions, especially in Africa, communication is carried on right through what would according to the theory be the skip distance of the very short waves.

The long wave work was discussed at one meeting. There was considerable discussion of the phenomena observed in transmission over moderate distances, on the relation of the intensity of the signals to solar activity and also on that curious variation in the intensity with distance which has been observed in England in which the intensity did not fall off regularly with the distance as had been generally believed but showed maxima and minima, apparently proving the existence of interference phenomena. This exists in England in the daytime, it appears, while in America we observe similar phenomena at night, but nothing of the kind in the daytime, and it seemed a matter of considerable importance to discover just what the difference in conditions are which produce the difference in behavior of the waves in the two countries.

The transmission over greater distances was also discussed. The question of the possible difference in the transmission from east to west and from west to east was mentioned, but apparently there is a considerable amount of conflicting evidence on this point and no conclusions were reached. In conclusion it was agreed that all of the work at great distances proves the great need of improvements in our methods of measurement and in our understanding of the degree of variation which is to be expected in the field intensity in different portions of the earth and under different circumstances of transmission and that the long distance problems can only be solved by a very large increase in the number of observing stations. It seems, therefore, desirable that all pressure possible should be brought to bear on the organizations which have funds at their disposal to establish such stations for a more speedy solution of our radio problems.

(*Signed*) L. W. Austin,
Chairman.

Report of Commission III.

La Commission a tenu quatre séances dans lesquelles elle a étudié un programme dont les grandes lignes étaient les suivantes :

Méthodes d'investigation. — Écoute et enregistrement des atmosphériques, radiogoniométrie, oscillogrammes.

Atmosphériques considérés comme phénomènes radioélectriques. — Nature des perturbations, leur fréquence, fréquence des oscillations qu'elles engendrent de préférence dans les récepteurs; brouillage des signaux.

Atmosphériques considérés comme phénomènes physiques. — Forme des oscillations qui les produisent; leur portée, leurs variations diurnes et saisonnières; leurs sources.

La discussion a surtout porté sur les sources possibles, deux théories opposées se trouvant en présence; celle de la grande portée et celle de la très faible portée. Aucune conclusion n'a été obtenue, mais des arguments de valeur se sont confrontés et ils constitueront une amorce précieuse pour une analyse serrée qui pourra avoir lieu, nous le pensons, lors de la prochaine session.

D'autre part, le comité britannique de l'U. R. S. I. avait soumis à la commission un programme d'observations internationales. La discussion de ce programme a conduit aux conséquences suivantes :

Des recommandations vont être envoyées à tous les comités nationaux pour qu'ils fassent le plus tôt possible des enregistrements leur permettant de déterminer la direction des atmosphériques et les instants exacts auxquels ils se produisent.

Les comités nationaux seront priés de se mettre en relation les uns avec les autres, selon leur possibilités pour assurer à ces observations la plus grande uniformité possible; mais pour ne pas retarder la mise à exécution du travail, ils l'entreprendront dès que possible avec les moyens dont ils disposent.

Pour toutes les autres questions, les comités nationaux seront priés d'étudier les détails de réalisation en se mettant en relation avec le comité britannique qui a déjà réalisé des appareils de grand intérêt.

En ce qui concerne le dépouillement des résultats, la commission estime qu'il est préférable de le faire d'abord séparément dans chaque pays, mais après entente avec les comités intéressés par les mêmes sujets de façon à faciliter l'étude des résultats.

(*Signed*) R. Mesny,
Chairman.

Report of Commission IV.

La IV^e^ Commission a tenu quatre séances, dans lesquelles on a décidé :

1° D'étendre son activité, comme il a été proposé par M. le Général Ferrié, aux liaisons avec les autres sciences (géophysique, astronomie, etc.) en adoptant aussi la proposition de M. Austin sur l'activité solaire;

2° D'accepter la proposition française sur les ondes courtes, et de charger M. Bureau et M. Goldschmidt d'en étudier l'application, comme aussi le travail des amateurs et expérimentateurs volontaires, en le standardisant le plus possible.

3° De demander à la Commission des Atmosphériques d'étudier, en commun avec la IV^e^ Commission, l'organisation du travail proposé par le Comité National Anglais.

4° De demander à la prochaine Assemblée Générale de 1928 de voter les moyens d'encourager les experimentateurs et amateurs dans leurs recherches, notamment par l'attribution de récompenses.

(*Signed*) Prof. G. Vanni,
Chairman.

General Ferrié. — Dr. Goldschmidt thinks it would be interesting to study the manner of distributing to everyone the results of work done in the National Committees and in particular graphical results of observations. Dr. Goldschmidt has an idea which consists in making a micro-photograph of these curves and distributing them in the National Committees. The curves would all be on the same dimensional scale. In this way we could in each country get to work on all the results obtained in other countries — more easily perhaps by superimposing curves obtained by various observers, and we could really have very interesting results. We should encourage Dr. Goldschmidt to consider this matter further and to make some proposal to the national committees.

We have now terminated our work. We have accomplished a great deal. Ideas have been exchanged between scientific men, and the results of their discussions will be printed and distributed for the greater benefit of science. We still need much more information, and that is why many questions we have discussed are still open, but we have already reached very useful conclusions. It will be necessary to extend these experimental researches and to coordinate them, in order to get the greatest profit possible. We have done here a practical work by the exchange of theoretical ideas. When we meet again in 1928 we will have a great many things to say and ideas to exchange, and I think our frequent meetings will increase our interest in the science which most appeals to us.

I wish to thank our General Secretary again for his activity and his wisdom and we all give him our thanks, and we expect from him in the future a great deal of service.

I also express our thanks to the American Committee for all the trouble they have taken to facilitate our work. The have attended all the meetings and have contributed to the discussions on all questions.

The session is closed, and we will meet in Brussels in September of next year.

* * *

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RECUEIL DES TRAVAUX
DE
l'Assemblée Générale tenue à Washington
en Octobre 1927

PAPERS
OF THE
General Assembly held in Washington
in October 1927

FASCICULE 3

Travaux de la Commission des Méthodes de Mesures et Étalonnages (TEXTES ORIGINAUX)	Papers of the Commission of Measurements and Standardization (ORIGINAL TEXTS)

PUBLIÉ PAR LE SECRÉTARIAT GÉNÉRAL DE L'U. R. S. I.
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Juillet
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International Union of Scientific Radio Telegraphy

PALAIS DES ACADÉMIES, BRUXELLES

Secrétariat Général : 54, avenue des Arts, Bruxelles

Recueil des Travaux de l'Assemblée Générale tenue à Washington en Octobre 1927

Papers of the General Assembly held in Washington in October 1927

TRAVAUX DE LA COMMISSION DES MÉTHODES DE MESURES ET ÉTALONNAGES

(Textes originaux)

La Commission des Méthodes de Mesures et Etalonnages de l'U. R. S. I. a été constituée par décision de l'Assemblée générale de l'U. R. S. I. tenue à Bruxelles en juillet 1922.

Les travaux effectués en cette matière par chaque Comité national ont été résumés et discutés, lors de l'Assemblée générale de l'U. R. S. I. tenue à Washington en octobre 1927. On trouvera dans ce fascicule 3 l'ensemble de ces discussions et rapports.

La Commission des Méthodes de Mesures et Etalonnages est composée des savants suivants :

PAPERS OF THE COMMISSION OF MEASUREMENTS AND STANDARDIZATION

(Original Texts)

The Commission of Measurements and Standardizations of the U. R. S. I. has been instituted by the decision of the General Assembly of the U.R.S.I. held in Brussels in July 1922.

The works made in such matter by every National Committee have been summarized and discussed, during the General Assembly of the U. R. S. I. held in Washington in October 1927. This part 3 includes the complete discussions and reports.

The Commission of Measurements and Standardizations is composed of the following savants :

Belgique : M. HENRIOT, Professeur à l'Université de Bruxelles.
M. le Commandant TRICOT, Chef du Bataillon de T. S. F. de l'Armée.
M. CORTEIL, Ingénieur Principal des Télégraphes.

France : M. ABRAHAM, Professeur à la Sorbonne.
M. JOUAUST, du Laboratoire Central d'Electricité.
M. BETHENOD, Ingénieur.

Great Britain : Sir Richard GLAZEBROOK, K. C. B.; F. R. S.
Mr D. W. DYE, B. Sc., of the National Physical Laboratory.
Mr F. E. SMITH, C. B.; F. R. S., Director of Research at the Admiralty.
Mr J. Erskine MURRAY.

Italie : Prof. G. VANNI, Directeur de l'Institut Central Militaire de Radiotélégraphie.
M. G. V. VALLAURI, Professeur à l'Université de Turin.

Norvège : M. L. VEGARD, Professeur de Physique à l'Université d'Oslo.
M. H. PETERSEN, Ingénieur supérieur, Chef de la Division Radiotélégraphique à l'Administration des Télégraphes de Norvège.

Portugal : M. le Capitaine Apolino RODRIGUES, Professeur à l'Ecole Navale de Lisbonne.

United States: Dr. J. H. DELLINGER, Chief of Radio Section, Bureau of Standards.
Prof. A. N. GOLDSMITH, Radio Corporation of America.

Les séances de la Commission, à Washington, étaient présidées par Mr D. W. Dye, du « National Physical Laboratory » d'Angleterre.

The meetings of the Commission, in Washington, were presided at by Mr D. W. Dye of the National Physical Laboratory of England.

Meeting of October 12, 1927

Dr. DYE, Chairman.

Dr. Dye. — The scope of the radio work classified under Commission I is very considerable since it embraces not only its prime business concerning radio measurements and standards of frequency, capacity, inductance, resistance, etc., but also concerns itself with the performance of those assemblages of apparatus used in research in the fields of Commissions II and III.

Reports have been drawn up by some of the National Committees embodying the work done under Commission I in their respective countries. No general report has however been presented by the Chairman of Commission I embodying the present international status of radio standards and measurements.

In the short time which has elapsed since my appointment as acting chairman of this Commission, it has been impossible to draw up a formal general report embodying all the large number of activities to be considered under Commission I. This however is probably of not much importance since the discussion will bring to the front most of the really important work.

In order, however, to suggest definite lines of thought, I have ventured to try and arrange so that the discussions follow fairly well defined paths. I suggest that at this afternoon's meeting the discussions be directed toward radio frequency standardization and measurement in all its aspects, including the uses which may be made of piezoelectric or other mechanical vibrators as secondary frequency standards.

Our discussions will include the subjects in the program for this Commission which was included in a plan prepared by the Secretary General, as follows :

a) Discussion of report of the Chairman of the International Committee;

b) Comparison of standards of frequency, with discussion of the methods to be employed. The employment of quartz resonators (Japan);

c). Discussion of the methods of measuring field strength. It is desirable that this discussion should be accompanied, if possible, by an experimental comparison of the apparatus employed;

d) Discussion of the methods of measuring decrement and phenomena of interference produced by radio stations using spark and telephony. This subject proposed by the British Committee seems outside the scope of the U. R. S. I. but as they are functioning up to a certain point as a technical committee of the International Radio Telegraph Conference it is thought the subject should be included;

e) Discussion on the procedure of measuring the speed of radio waves at various frequencies (Japan).

Let us first consider the subject of frequency standardization. The continual filling in of the frequency spectrum by the ever-increasing number of transmitting stations has enforced an increase in the accuracy and constancy of the frequency of opreration. This has already reached the stage in some regions of the spectrum where variations of ± 0.1 % are undesirable. The station substandards of frequency should therefore have an uncertainty not greater than a few parts in 10,000. The more primary standards and the national standards must therefore be again more accurate than this. It is not aiming too high to suggest that one or two parts in a hundred thousand should be aspired to at the primary sources of radio frequency standardization.

These considerations rule out entirely all those secondary forms of standard based on calculations and measurements of capacity and inductance or on the constancy of such circuits. Any system based on length measurements of short waves, however accurately this may be done, must also be considered secondary since wave length is a function of the medium and when the waves are guided along wires the wave length may vary quite considerably according to the material and dimensions of the wires.

I think therefore that it will be unanimously agreed that the reciprocal of time must be our unit, and that our radio frequencies must be linked to this unit as directly as possible.

We are in a more fortunate position with respect to the unit that in any other branch of measurement. At any point on the earth's surface a day has exactly the same duration and by the help of high class clocks we all have waiting to be used in our laboratories the same seconds in terms of which our frequencies are to be measured.

We have not therefore to provide ourselves with an international unit such as we have for electrical resistance and current so that intercomparisons of the unit become unnecessary.

It is in the building upwards in frequency from the time unit or downwards from a radio frequency to it that possible sources of error may creep in; also in stepping off from the standard when actually measuring a radio frequency, the errors are not generally quite negligible.

One of the outstanding features of the developments of the methods of radio frequency standardization in terms of the second has been the almost universal adoption of a harmonic principle whereby associated circuits have currents produced in them which are exactly harmonic to one another in frequency.

Various frequency multiplying devices have been developed in a very satisfactory manner so that it is possible to obtain at the output end of a system a radio frequency of 10, 100, 1,000 or even 10,000 times the input frequency (usually in the telephonic region). The essential feature of the system being, of course, that the ratio of the frequencies is a known and quite exact integer.

The measurement of the telephonic frequency can be made with great precision (assuming sufficient constancy) by the help of some form of phonic wheel and chronograph. There is also another method of which I may be enabled to speak later.

In those laboratories which are in a position to provide themselves with the necessary equipment it is no longer necessary for them to have international frequency comparisons for the purpose of checking their standards. It is however of considerable interest to see what degree of agreement is obtained in the assigned values by different laboratories to the frequency of some form of portable standard source of radio frequency of sufficient constancy in order to see just what the errors associated with the measurements are.

I shall say nothing at this juncture regarding such comparisons, since Dr. Dellinger who has been responsible for the initiation of a number of such intercomparisons will speak of them tomorrow afternoon. It seems to me unnecessary to make intercomparisons from the point of view of actually observing errors in the various National standards because I think the standards in any one large laboratory are more accurate than the comparisons which can be made. I think it is unnecessary to make intercomparisons of frequency from that point of view, but they are, of course, very interesting. I will therefore conclude by suggesting that the discussion be opened from the point of view of the following matters.

1. International comparisons of radio frequency standards.

2. Dissemination of such standards whether by portable self contained standards or by the broadcasting of standard frequency waves.

3. The provision and standardization of the radio frequency standards of those countries who are not in the position to, or do not contemplate providing themselves with primary radio frequency standards.

Dr. Dellinger. — I find myself in excellent agreement with all that Dr. Dye has said. I think he has summarized the situation very well, and, as he has pointed out, it is of considerable interest that we have arrived by natural evolution at agreement in values and also in methods in the various countries. The method of using harmonics to step up from a low frequency which can be very directly related to the measurements can not only be made very readily but it is possible to step up from the operation of a clock pendulum by a direct process to a radio frequency or to step in the reverse direction so that it is even possible to use our standard of time directly as a standard of frequency. Dr. Dye has himself developed a method of doing it, a method has just recently been developed in Italy by Prof. Vallauri, and I understand that Mr. Horton will also present a method of that kind at our session tomorrow (1). This being true, that we can step so satisfactorily and as desired to radio frequencies rather settles definitely the wise policy and suggestion of Dr. Dye that the unit of time shall be the basic unit in terms of which frequency is measured. The rotating earth becomes our standard of radio frequencies and therefore it is not necessary that physical standards be considered as primary standards of radio frequency.

I am not entirely in accord with Dr. Dye to the effect that international comparisons are unnecessary. In any physical measurement it is necessary that there be standards, even though the standard can be directly related to the unit which it represents. At any one time it is quite desirable that there be in existence somewhere a standard in case of disagreements, and similarly even though we are able to measure with extreme accuracy in the different countries now, I believe that it is desirable from time to time that international comparison be made so we can determine by actual demonstration that we are measuring with the accuracy with which we think we are measuring.

Another subject which has been placed before us is that of providing for the countries which do not themselves have means for making measurements with the highest precision so that the very accurate frequency standards can be made available to them. You may know that there has been held within the past few weeks a meeting in Paris of the International Committee on Weights and Measures which has been considering this very same question in regard to the electrical units and standards, and the American representative at that meeting, I believe, is presenting this very question in regard to the frequency standards as well. I do not know what is the best solution for that. It is possible that the International Bureau of Weights and Measures could maintain certain radio standards, the value of which could be determined by measurements of precision in the various large national laboratories and then that the comparisons could be made at the International Bureau of any standards sent to them by other countries. I am not sure that this could be done. It is rather a difficult job. That is a question worth further discussion and one which is of considerable importance. It is not only necessary that in each country we have a satisfactory frequency standard but that the whole world have a satisfactory frequency standard.

On the subject of the accuracy obtainable, it is true that one part in 100,000, which is a very considerable precision, is nevertheless apparently an attainable goal. In several of the countries frequencies are now being measured to better than one part in 10,000. In this country, America, it happens that a number of organizations, commercial and Government, came together on a program to realize frequency standards to a part in 100,000. We have not yet arrived with certainty at that goal but we believe that before many months we shall, and that goal will be attainable before any great length of time throughout the world. So far as the basic measurements are concerned we are in sight of an extremely satisfactory situation on frequency standards.

The question of dissemination with that accuracy so that radio stations can hold their frequencies is another question and one of administrative character, our concern being only that of seeing that instrumental means are available for doing it.

General Ferrié. — I find myself in complete accord with the views that have been expressed by Dr. Dye and Dr. Dellinger.

We have come to a universal belief that the rotation of the earth is the essential basis of our determinations of frequency, only unfortunately the astronomers have come to an agreement upon the statement that the velocity of the earth's rotation is not perfectly uniform but that it suffices that it is sufficiently uniform for the needs of our particular art if we are satisfied with one part in 10,000. I think there will be no difficulty in arriving at complete accord. We are slaves of time, and particularly are we slaves of time in this particular radio art.

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(1) Cf. fascicule 1, p. 28.

Dr. Dye. — I am not in disagreement with Dr. Dellinger on the subject of international comparisons of frequency. What I wished to say is that I do not believe it is necessary to have these comparisons from the point of view of ascertaining the inaccuracies of our primary standards. We can by such comparisons discover with what accuracy we actually measure frequency.

Dr. Dellinger. — I might put it this way. International comparisons have the effect of making frequency measurement a cooperative investigation by all the countries.

Mr. Yokoyama. — It is very important at this time that radio comparisons and tests be carried out on an international scale. The first step to attain this object will be, I think, the use of the quartz oscillator, though quartz itself seems to have some properties still unknown to us. Therefore the Japanese Committee expresses the desirability of having standard piezo oscillators distributed to different countries adhering to the Union for facilitating the intercomparison of frequency. The oscillator should be standardized at a central bureau and distributed with the certificate, just in the same manner as countries using metric system have national prototypes of meter and kilogram.

We have received a quartz oscillator from the Bureau of Standards to calibrate its natural frequencies as an oscillator by our frequency standard, but Japan has not yet established a national prototype for the frequency. We, therefore, carried out the calibrations at the Navy and Communications Department independently. These calibrations seem to be accurate to better than within one tenth of one percent, which will be sufficient for the beginning of international standardization of radio frequency.

Prof. Mesny. — There have been some experiments made in France with quartz piezo oscillators but in order to get maximum use from them it is desirable that they should be developed as practical frequency standards. Sometimes frequency meters are not entirely satisfactory in regard to the precision of their operation, and it would be desirable to give special attention to piezo oscillators as practical frequency meters.

Prof. Vanni. — In the Italian Marine Laboratory at Livorno, measurements have been made of the frequency obtained from a piezo oscillator received from the Bureau of Standards by what might be called absolute measurements. Prof. Vallauri has found a very remarkable degree of agreement between the frequency as obtained from the quartz and the frequency as obtained from the fundamental methods of measurement, but when in practice one compares frequency standards by different frequency meters measured and calibrated in laboratories the comparative precision that they show is much less than that which can be obtained under favorable conditions from the quartz oscillator if the quartz oscillator gives one part in 10,000. Even the precision frequency meters of excellent laboratories agree to only several parts per 1,000 or perhaps a fraction of 1 %.

There is much to be done, therefore, in order to agree upon methods that shall be used as a common basis for fixing upon the standards of frequency in all parts of the world. At the present time the Italian Marine has distributed quartz plates to different laboratories in Italy with satisfactory results, but the main problem has yet to be solved.

Prof. Mesny. — There are two separate questions involved : first, the comparison by means of quartz oscillators of the standard tuning forks of two different laboratories (and measurements have shown that the tuning forks are in close agreement); second, the maintaining of frequency meters which are used for practical application of those frequency standards. It is open to question whether the indications of practical frequency meters can be relied upon continuously without variation for any length of time, and the variations which are found in these wavemeters are much greater than those which present themselves with the first set of experiments with tuning forks, so that an important question is finding wavemeters that can be relied upon to maintain indications uniform and to repeat themselves satisfactorily from one place to another.

Dr. Dellinger. — On this subject, any laboratory which is to maintain frequency meters for practical purposes necessarily must have some standard which it uses as its own basis. Now the advent of the quartz plate has given us a form of standard which has become pretty near universally used for that purpose. Any laboratory which makes any pretension to maintaining frequency standards with considerable precision is pretty likely to have quartz oscillators or resonators at the present time. I think therefore that it would be quite desirable that we have a considerable discussion of that subject.

One or two years ago many people jumped to the conclusion that we had in quartz a standard which would maintain its value with absolutely unchanging fidelity. Considerable experimentation has now gone on and the limits of the quartz oscillator and the quartz resonator are fairly well known. A number of the gentlemen present have done that work and we are in a position now to say to what extent the temperature variations introduce inaccuracies and to what extent the variations of circuit constants and the method of mounting the particular piece of quartz used and other factors, to what extent those things affect the accuracy, and I am sure that we could all profit by an exchange of experience on those points.

Now that we have come to the point where we can use piezo oscillators with the temperature of the quartz plate controlled and those other factors largely controlled, we are also finding that the accuracy of the measurement depends either on the precise method of coupling of one piezo oscillator to another, or in the indicating device in the process of measurement, and to go to an accuracy of one part in 100,000 or greater that point must be studied further, and I should be very glad to hear further discussion along those lines.

Prof. Terry. — I have an investigation under way to determine the extent to which the different methods of coupling the crystal in the circuit affects its frequency. It is largely a theoretical investigation. I have used the following different circuits : 1) Crystal between grid and plate with resistance coupling in plate circuit; 2) Crystal between grid and plate with inductive coupling in plate circuit; 3) Crystal between grid and plate with tuned plate circuit, and 4) Crystal between grid and filament with tuned plate circuit. My results indicate that with the same crystal in the same mounting and at the same temperature there are quite marked differences in the frequency that it will give. If we consider the equivalent electric circuit for the crystal, what we really have is a coupled circuit, the crystal being one part and the circuit the other, and the period which you get is one of the periods of the coupled system. The amount by which the crystal is forced off of its natural period of oscillation depends upon the circuit that is used and the degree of coupling. My results indicate that even if we want to get to even one part in 10,000 we must not only specify temperature and the type of mounting of the crystal but we must also specify the circuit in which it is to be used.

Dr. Goldschmidt. — How is the crystal considered as an electric circuit?

Prof. Terry. — I have considered the equivalent circuit as given by Cady, an inductance, a resistance, and capacity in series, the whole thing shunted by another condenser.

Dr. Goldschmidt. — What precision have you been able to rely on, taking into account these modifications of the circuit by means of couplings?

Prof. Terry. — I have not proceeded with the work far enough to answer that question. So far it is largely a theoretical investigation and I am attempting just at the present time to check it up experimentally; I am having some difficulty getting a quartz crystal that will operate equally well in the four different circuits that I have investigated.

Dr. Goldschmidt. — I desire to express my compliments on the work which you are doing.

Dr. Dye. — I should also like to compliment Mr. Terry on the work which he is carrying out on the question of the maintenance of oscillations of quartz crystals and to express the hope that he will arrive at some satisfactory mathematical conclusions. I have also made some investigations along this line and the simple equations indicate that the system ought not to oscillate.

Dr. Taylor. — The question which Dr. Goldschmidt asks as to ultimate precision attainable has concerned us in the United States Navy very closely. I do not care to go into that in detail because papers will be presented at tomorrow's afternoon session which will try to show how far we have gone, what precision we have actually attained, and what precision seems to be in sight, but I can anticipate so far as to say that it would appear that with a given circuit, a given tube, with temperature control within a reasonable amount, and with a very carefully worked out mounting of the crystal, that 1,000th of 1 % on primary standards should be wholly reasonable.

Now, I was interested in what Prof. Mesny and Prof. Vanni said concerning difficulties in getting measurements to agree when they are made by different methods. You may be interested in the method which we follow in our own Naval Service. The measurements are carried out on board ships. We have frequency meters which are supplied with quartz oscillator attachments in fixed mounting, with fixed tubes and fixed circuits. This permits us in a few minutes to check accurately the calibration curves of any heterodyne frequency meter. We do not expect these frequency meters to show an order of accuracy higher than perhaps 0.05 % unless they are very frequently calibrated, but when they are used for very accurate work they are always calibrated. In that way we continually correct the curve of the instrument, and we believe that we get results approximating very closely 100th of 1 % by that method, and they are very consistent on different ships. We have a large number of these wavemeters in active service and they are giving excellent service. We do not place our reliance solely on the wavemeter or the heterodyne frequency meter but we reenforce that with a very carefully arranged crystal calibrator. At present one of the standard calibrators is 50 kilocycles and we have others at 500 kilocycles, etc., suitable for different ranges of frequencies. If methods of that sort are universally followed I think many of the discrepancies mentioned by Prof. Vanni and Prof. Mesny will disappear. It seems to me that each country must have the best primary standard it can get, which should be very nearly as accurate as the second is. I should like to ask General Ferrié how much variation we may expect in the value of the second because we may be able to run down some trifling errors which we have in standards we have recently been comparing. They may be due to variations in the second. I think that the different countries should intercompare their primary standards in order to keep each other from going wrong. Each country must have its actual working secondary standards, and if this organization can pave the way to better methods of using these secondary standards I think we will have accomplished a great deal.

General Ferrié. — Astronomers find that there are certain deviations in the assumed places of the stars and that these deviations may be attributed to variations in the rotation of the earth. Nevertheless we ought not to attach too much importance to that possible variation because it is clear that in the course of one day the precision attainable over a period of 24 hours in time approximately down to 100th of one second which is greater than one part in a million and therefore could hardly be expected to produce variations of the order that have been mentioned. I think that this matter is one of interest to all who are concerned in the measurement of time and it is a refinement that need not be taken into discussion for the moment.

Although the variation from uniformity of the earth's rotation may be considered as outside of our present order of precision, nevertheless the practical question of maintaining uniform subdivision of time after the astronomer has made an observation of it at a given instant is much more serious because the error of the clock enters into the whole subject. Measurements which have been made at Paris show we ought to take extraordinary pains to allow for variations in the clock rate during the period of time covered by the experiments, this being much more serious, and in the paper that I will read tomorrow at the public session (1) the measurements therein described indicate how we have been able to overcome these errors.

Dr. Taylor. — The General may be interested in knowing that we are well aware of those difficulties and we are experimenting with a type of clock driven by a piezo oscillator to avoid these errors.

General Ferrié. — In all cases one has to consider the effect of seasonal disturbances and that may be quite serious in several cases, but I hope that only in very rare instances are these errors sufficiently great to come into these determinations.

Dr. Van Der Pol. — I have done some theoretical work also on a given circuit with quartz oscillator and one can easily see how this constancy of frequency is maintained. A given circuit of a piezo quartz oscillator is composed of self inductance, capacity and the resistance in series, the whole thing being shunted. The first capacity is I find in some experiments to be of the order of 100th of 1 centimeter. We have to do here with a circuit of very small capacity and unusually large self-inductance, and it is easy to see whenever you put a piece of wire in series it can hardly alter the frequency because this wire has an inductance very small in comparison, and in the same way shunting it by a capacity does not alter the frequency very much because of this frequency being big. Now the fact that such a circuit maintains its frequency so well, I think, is due to two facts. The first is this, that the shunted big capacity is large compared with the internal small capacity so that you can get at the thing electrically only through knowledge of a capacity. When you couple this quartz oscillator to some tube the coupling is of the capacity potential divider and is always very loose indeed so that it does not matter how you coupled this oscillator to some circuit the coupling is always very loose and the oscillator maintains its natural characteristic very much more than a circuit would where it has to be coupled very much closer to a tube in order to maintain an oscillation. So the coupling of this circuit is always very loose. One has to take into account the internal capacity of the triode, and internal capacities are of considerable importance in connection with the behavior. We find theoretically and also find it confirmed experimentally, especially when a change of frequency caused by the external circuit occurs at the moment when the external circuit is nearly in resonance with the proper natural period of the quartz oscillator.

Prof. Terry. — I would like to point out in that connection the internal capacities are of the same order of magnitude as the large capacity and in my work I have taken into account all three of the internal capacities.

Dr. Dye. — In connection with these theoretical considerations no mention has been made of the capacities which exist between surfaces of crystal and electrodes. These are very important and are the cause of most of the variations in frequency.

Prof. Terry. — I put a very thin coating of silver between the electrode and the quartz.

Dr. Goldschmidt. — Dr. Goldschmidt points out that in the ordinary method of silvering, it has been found that the surface is rapidly injured by the vibration of the quartz but perhaps there may be other methods of applying the silver in such a manner that the surface will hold and not subject to destruction.

Prof. Terry. — I have used no special precautions. Just the ordinary silvering process we have used in the laboratory, the Brashear process.

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(1) Cf. fascicule 1, p. 21.

Dr. Dye. — I should like to point out that the effect of silvering is to compel that surface to be an equi-potential surface which it will not be at all in the absence of that covering. There are often points near together on the surface with differences of potential of 100 volts.

Dr. Van Der Pol. — I should like to know how these results compared with the results obtained by Scheibe in Germany where they had some quartz oscillators in a vacuum with some helium and this helium showed spots of high potential. I think these potential spots were quite where you would expect them according to classical theory.

Dr. Dye. — It is quite true that in Dr. Scheibe's resonators the vibrations are according to the theory. I think that in all of the cases shown by him the specimens were in the form of long rods in which the dimension in direction of vibration are large, and that is a case where a plane surface may be expected to remain plane. When the shape of the specimen is such that all the dimensions are similar, then the vibrations are different and it is difficult to say what the mode is.

General Ferrié. — The interesting remarks are in conformity with some results recently developed in France by Prof. Tawill where by means of polarized light upon the quartz it is possible to observe the vibration patterns, and in this way it is very easy to follow the vibratory action. It is not always constant; it varies with the harmonic present in the electric circuit, but otherwise the agreement is fairly well established with what might be called the fundamental theory of vibration.

Dr. Dye. — I have read that paper too. I think all the observations recorded there have been made with respect to longitudinal vibrations.

There is plenty of field for further work in the investigation of the behavior of piezoelectric quartz crystals when oscillating in various modes of vibration. Such work is in progress in many places.

If we are not considering whether we should define some form of standard we might consider other means by which radio standards can be disseminated, for example, a large number of countries transmit waves of standard frequencies which are received at the various countries and are used for calibrating purposes. These have some advantages over the quartz oscillators because they are so broadcast and available to all, and I think it should be considered whether there are not means by which international comparisons can be made by standard waves so that they can be received over very considerable distances. There are available in England some stations which are controlled in a manner which might be considered to give them a standard frequency and it may be that there are stations in America or in Europe of sufficient power and perhaps controlled in some way that gives them a high degree of accuracy so that they could be used for international comparisons.

Dr. Goldschmidt. — By short waves perhaps it would be possible to obtain a large radius.

Dr. Dye. — Such short wave stations are also comparatively easily controlled by quartz oscillators which have the necessary degree of stability. So long as a reasonable degree of simultaneity is observed in the various countries making the observations they need not be just exactly synchronous in time.

Dr. Vanni. — What are the indications at the present time of the large standardizing laboratories of the world, such as the National Physical Laboratory and the Bureau of Standards, of making measurements of the waves of such short lengths as 5, 10 and 15 meters?

Dr. Dye. — At the National Physical Laboratory we have no difficulty in carrying our measurements of frequency up to that represented by 5 meters wave length to exactly the same accuracy as we can measure any other frequency. It is merely a question of using a harmonic oscillator to carry the frequencies up beyond the range given by the wavemeter which we have. The linking on is done by harmonics and presents no difficulties whatever.

Dr. Dellinger. — I would like to say the same thing for our experiments in the Bureau of Standards. The method of harmonics has been tested repeatedly and found to be free from error. It is not at all troublesome to get the 200th or 300th harmonic of the quartz plate.

As to the checking of such frequencies by an independent method, about two or three years ago at the Bureau of Standards we were utilizing the method of measuring wave length directly on a number of parallel wires, and for the accuracy with which we were working at that time, namely limited to about one part in 1,000, we found that the measurements did check. In the last few years, however, which has been the period in which the accuracy has been pushed up to greater precision we have not used that method and believe that it is not susceptible to give an accuracy as great as with the quartz plate.

Dr. Dye. — I am in agreement with Dr. Dellinger in regard to the uncertainties if an accuracy greater than one part in 1,000 is desired when using parallel wires with standing wires. Messier carried out some very careful experiments using parallel wires observing standing waves on these and also at the same time measuring frequency by a harmonic system and in that way determined accurately the velocity of the propagation of the waves. This showed that one could not rely on that method unless the corrections were made.

Dr. Dellinger. — While this method is now only of historical interest, still there are some points worth mentioning. Dr. Hund of the Bureau of Standards worked out from theoretical considerations a correction factor which takes account of the resistance of the wires, the distance apart, and other factors which are susceptible of calculation; the method can be increased in accuracy by calculating these effects in advance. But even after applying these corrections it has been found in our experimental work that measurements made at different wave lengths starting from a different selected wave length do not agree, even after these corrections are made, to better than about one part in 1,000, and while we have never traced down with certainty the cause of the remaining discrepancies it seems likely that they are due to the surroundings. Therefore it has not seemed worth while to pursue the matter further.

Dr. Van Der Pol. — It might be possible perhaps to repeat this experiment with concentric tubes instead of parallel wires. In this way you would get rid of the effect of the surroundings.

Prof. Terry. — Prof. Pierce of Harvard is working on that at this time.

Dr. Taylor. — To return for the moment to the suggestion of international frequency comparisons by simultaneous measurements of signals from stations which are heard over great distances, it seems to me that a start can be made on that work any time. There are plenty of stations in the world today that are controlled by quartz crystals and can be heard literally round the world at appropriate times, at least with a sufficient audibility to get pretty precise measurements on the frequency. The Navy is transmitting from our stations here in Washington time signals at least two different hours of the day on four simultaneous frequencies, 4,015, 8,030, 12,045, and 16,060 kc. These stations are at the appropriate hours heard in very wide ranges and they are of piezo-electric control. The arrangements for holding the temperature of the crystal constant are by no means perfect. The crystal temperature is held within a fairly narrow range but it would be a rather easy matter to change that so that the temperature of the crystals would be quite constant if it were worth while, but if the Naval stations in this country can be of any service in making such an arrangement to control the temperature of the crystals to get more constant frequency than they now have, we would be glad to undertake that since they can be heard in so many places and those frequencies could be simultaneously measured by the Bureau of Standards, by our laboratory, and by your laboratory, and if the regular transmissions of traffic are not suitable we would be very glad to undertake special transmissions for any government, I am very sure.

General Ferrié. — The same thing that Dr. Taylor speaks of might serve for other purposes : for instance, to determine whether the effective time of propagation is the same upon all these four frequencies. If different antennas are employed and they are not all energized at the same instant it could be the object of observers in different parts of the world to see whether the signals recorded maintain the same relative differences of time between one another or whether these differences are affected by various circumstances. Then again, if the frequency is controlled great precision in the manner described by Dr. Taylor, it would also be possible to determine time absolutely at different parts of the world and to control the clocks in the stations. So there are several excellent results could be obtained from this method besides the determination of frequency.

Dr. Taylor. — The accuracy of such measurements at this time is about 100th of 1 %; I think this is satisfactory; the requirements of our own service are fully that high. General Ferrié may be interested to know that we are aware that time signals do not arrive exactly on the same time on the short waves as on the long waves nor on two short waves which are somewhat different from each other. It is pretty generally agreed now that the higher frequencies travel through a higher region of the Heaviside layer. It would indeed be very interesting to do further work on these lines and find out just what differences might come out of it which would give us a good deal of additional information as to the structure.

If one listens to a signal which has passed clear around the world, which is quite commonly experienced with a number of us, one can actually hear these different echoes come in, though it is quite clear that the signal has arrived by several different velocities and an oscillographic analysis such as taken by Dr. Qäck in Berlin shows that the signal has had a very complicated past history and has traversed the path around the world by several different routes, and that different portions of the dot and dash have different apparent velocities.

Mr. Shaughnessy. — I should like to propose, as Commander Taylor has offered to do this work, that the Union should accept his offer right away. He is prepared to do all the work, and I am sure it will be useful to all the nations.

Dr. Taylor. — I suggest the frequencies should be measured for the United States by Dr. Dellinger's laboratory.

Dr. Dye. — I wish to express thanks to Dr. Taylor for his generous offer to place his station at the disposal for that purpose and his willingness to improve it if it is found necessary, and especially in view of the remarks of General Ferrié that such signals would be useful for other purposes I should like to ask whether General Ferrié would like to make some resolution on this.

General Ferrié. — I am entirely in accord with the opinions and suggestions expressed. In France there are two stations which give time signals, Bordeaux (18,000 m) and Issy-les-Moulineau (27 m). These are very widely separated on the scale of frequency or wave length, but they are not controlled at the present time by quartz plates. It may be possible later on to do this; if they are controlled to a sufficient degree of precision they may serve with the stations mentioned by Dr. Taylor.

Mr. Shaughnessy. — We have the Rugby station which is controlled by a tuning fork and the temperature of the tuning fork is also maintained reasonably constant and shortly we shall be sending out time signals twice a day over the Rugby station on about 16,000 cycles. Dr. Dye can say how constant Rugby is in frequency as he frequently measures it, and I think these signals also would be available for comparison of standard frequencies.

Dr. Dye. — On all occasions on which I have measured it the frequency has been within one part in 10,000. Does someone care to make a resolution?

Prof. Vanni. — Perhaps it would be well to place on record a desire on the part of this assembly that this kind of emission should within reasonable limits be multiplied in different parts of the world. For example, I have in mind an Italian station that has been given up for purposes of radio telegraph communication but which it is hoped may serve for an experimental station for emissions on short waves and perhaps an emission could be found intermediate between the two of General Ferrié which might serve a similar purpose.

Dr. Dellinger. — It might be a good idea not to form a resolution at today's session but to take today's discussion as a basis for further informal consideration among the members and set the next meeting of this Commission as a time for formal resolutions.

General Ferrié. — Thinks that the plan suggested by Dr. Dellinger is very good, that as we shall have to consider certain national proposals that may come before this body shortly it might be well to take this into consideration as well as what we have been discussing this afternoon before coming to a final conclusion and putting a final resolution on record.

Meeting of October 18, 1927

Dr. DYE, Chairman.

Dr. Dye. — At our first meeting, October 12, we discussed at some length questions relating to radio frequency standardization in its various aspects. Much valuable information was elicited from many of the members of the Commission. This information was augmented by several of the papers read on Thursday afternoon last (1).

The discussion had reached a stage, at the adjournment of the meeting, at which the means of accurate intercomparison of radio standards of frequency and their dissemination were under consideration.

Two lines of action were in view as follows :

1. The continuation of the distribution and circulation of piezo oscillators and resonators.
2. The use of standardized waves of suitable kind.

The possibilities of the piezo oscillator from this point of view are now fairly well established but much remains to be done in the development of the theory of its action.

The piezo oscillator is a striking example where practice has gone far ahead of theory.

Although the oscillator has received most attention, the uses of resonators should not be lost sight of. In some respects they have considerable advantages over oscillators. These are as follows :

1. They are very portable and their small bulk renders their transportation very easy.
2. By cutting the crystal so that the resonators have the form of a rod in which the dimensions along the length are considerably greater than those transverse thereto; the resonant frequency is almost entirely independent of temperature.
3. By making observations on the crevasse in the resonance curve of current in an oscillatory circuit to which the resonator is connected; the determined resonant frequency is entirely independent of the constants of the circuit, which is then reduced to its simplest form.

With regard to the use of standard transmissions we have received the kind offer of Commander Taylor to utilize the time and other signals sent simultaneously on four different wave lengths from crystal-controlled transmitters at the Naval station. The possible use of the time signal transmissions in connection with observations of the velocity of propagation of waves and with respect to astronomical time has been pointed out by General Ferrié and merits further discussion.

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(1) These papers are published in } fascicule 1.
Ces travaux sont publiés dans le } fascicule 1.

These waves are of high frequency. At the other end of the scale we have at Rugby in England the high power low frequency station in which the transmissions are built up in frequency and power from a valve maintained tuning fork. The constancy of frequency of this station may or may not be as great as that of the short wave crystal controlled stations but in any case it is constant enough for hours at a time to enable highly accurate intercomparisons to be made.

Although in each case the master control by a continuously running mechanical vibrator should ensure continuity in the phasing of the radio frequency wave, in spite of the keying, when sending Morse, it will probably be more accurate to use a continuous dash if that can be transmitted for a short interval.

In this connection I will read with the Secretary-General's permission, a letter addressed to him by the Chairman of the Japanese Radio Commission.

(COPY)

NATIONAL RESEARCH COUNCIL
(Department of Education)

Tokyo, May 24, 1927.

Prof. R. B. Goldschmidt,
General Secretary of U. R. S. I.,
Brussels.

Dear Sir,

. . . .

Concerning the various items of discussion at the Congress mentioned in your letter of March 4, the Japanese Radio Commission expresses the desirability of having standard piezo oscillators distributed to different countries adhering to the Union for facilitating the intercomparison of frequency. The oscillator should be standardized at a certain central bureau, and distributed with the certificate, just in the same manner as countries using metric system have national prototypes of meter and kilogram.

As to the means of measuring apparent velocity of electromagnetic waves, it seems desirable that time signals be transmitted on long and short waves. In Japan, signals from Bordeaux are automatically recorded at the Astronomical Observatory; they promise to give valuable results as to the propagation of long waves from west to east over land; if we can receive similar signals from America, they will contribute to our knowledge of the transmission in the opposite direction over wide expanse of sea water. If it is possible to get these signals also in short waves, many unintelligible facts may perhaps be cleared in course of time. The Japanese Radio Commission wishes these points to be discussed at the coming Congress.

. . . .

(*Se*) H. Nagaoka,
Chairman of the Japanese Radio Commission.

Mr. Minohara. — For the research on methods of measurement of frequencies, piezo oscillators are the best. For comparisons it would be better if the time signals were transmitted on long and short waves simultaneously, controlling the frequencies, in order to keep them constant, by means of the piezo oscillator. This is exactly the same opinion as we have but naturally this transmitted signal should arrive at all the countries of our Union with sufficient intensity. For our part, we would suggest Bordeaux in Europe and San Diego or some other station in America as the transmitting stations.

We receive the time signals from Bordeaux at the Bureau of Astronomy every day very easily and Mr. Hashimoto has already reported the results, which we have presented to this assembly. But as to the transmissions from San Diego, it is desirable that the power be increased a little in order that we may be able to make studies of the frequency, propagation, etc. We have received San Diego only three or four times in the department of communication, and it was found that the intensity was not sufficient to make measurements on the electromagnetic field.

If you agree, and these propositions are carried out, we will be very happy as we can then make studies of frequency measurement as stated in our proposition. But the goal is the same. At the same time with this program we could satisfy the second proposition which has already been communicated by letter. That is to say :

« As to the means of measuring apparent velocity of electromagnetic waves, it seems desirable that time signals be transmitted on long and short waves. In Japan, signals from Bordeaux are automatically recorded at the Astronomical Observatory; they promise to give valuable results as to the propagation of long waves from west to east over land; if we can receive similar signals from America; they will contribute to our knowledge of transmission in the opposite direction over a wide expanse of sea water. If it is possible to get these signals also in short waves, many unintelligible facts may perhaps be cleared up in course of time. The Japanese Radio Commission wishes these points to be discussed at the coming Congress. »

General Ferrié. — It will probably be possible to satisfy the request of the Japanese delegate. Bordeaux emits signals on two wave lengths, 19,000 and 27 meters. This has been done for one year, but the wave lengths were not controlled. The 27-meter station we shall be able to control by means of a quartz plate; and as regards the 19,000 meters, we shall at least maintain its frequency as stable as possible and accurately measure its actual value during each transmission.

Dr. Taylor. — We are already transmitting these time signals from Washington on six different frequencies. The station at Annapolis, call letters NSS, transmits on about 17 kc. One of the Arlington stations transmits on 113 kc, and then there are the four high frequencies going out at the same time that have been previously spoken of here. It is likely that the first Japanese communication was made before we considered those high frequency time signals. They have only been going a few weeks and they may be able to take advantage of those high frequency transmissions, but it may be that the hours of the day at which these time signals are sent are not suitable for the best reception in certain points. I think it would be quite possible, if this body requested it, for the Navy Department to arrange for the transmission of additional time signals at other hours suitable for reception on various frequencies. We did that nearly two years ago for the Australian Government. We transmitted some signals on about 4,000 kc at their request and on that frequency we had to choose the hour 0.600, or. 60' clock in the morning, and sent special time signals in order to have them effectively received and automatically recorded in Australia, so in working between two points on the globe which have a time difference it is well to choose the time when you send the signals to fit the post of observation, and that I think we can do. If there is any request made for special time signals for any scientific purpose, I think that the Navy Department will be very much inclined to grant it. Concerning the power of the San Diego station which is mentioned here I think it very unlikely that that will be increased at the present.

Dr. Dye. — With regard to these signals I think it is quite convenient to consider the question of time signals for astronomical purposes quite distinct from those for frequency intercomparison. Those signals would presumably be some form of short impulse sent according to some code but for frequency intercomparison I imagine it will be desirable to have a rather long continued dash lasting perhaps for a minute or something of that kind. Will it be possible for such a signal to be transmitted for that purpose?

Dr. Taylor. — I think as far as our service is concerned it would be very easy. I should suggest that we start out on the time signals and when you are ready to make your measurements if you notify us we will instruct the operators to lock the key for, say, 30 seconds. We would prefer to lock it for a series or so, 15 second intervals or something like that, to give the tubes a rest between, and give a series of say, 10 or 15 second dashes, then rest, and then another, and we could follow the time signals with some such program. It would take very little time and it could be very easily arranged.

Dr. Dye. — I think the details of the program can be settled easily.

Dr. Hulburt. — I would like to ask regarding the question of time signals — is that so that the various countries can find out what time it is, or is it as a means of study of propagation of waves around the world, or both?

Dr. Taylor. — Both. I would suggest that some attempt be made first to see what can be done on the time signals as they now are and then use that as the basis for a request a little later, for perhaps time signals at a special hour, and as for the frequency measurements we can send them out almost any time that they are requested of us. I think that the matter could be handled by correspondence or by cable.

Dr. Dye. — I am inclined to agree with Dr. Taylor that it might be desirable to carry out further measurements on these signals as they are because I imagine that in the case of making use of them in variations of speed in propagation it would be necessary to work out in detail experimental arrangements at the receiving end in order that the sequence may be preserved from one occasion to another occasion. That may have been done but considerable experimenting will be required before the full use can be made of them for the purpose which General Ferrié has suggested they might be used.

Dr. Taylor. — The high frequency transmitters at Arlington are some of them modulated with 500 cycles. We are arranging that so that they can transmit the pure unmodulated wave which would be much more desirable for frequency measurements. You can get it more exact with the pure unmodulated wave than you can with the wave as it now is, modulated at 500 cycles audio frequency. That change will take a little time and also we want to put more perfect temperature control on the crystals to hold the frequency still steadier so that if we start with the signals as they are and see what we can do, then it will give us a chance to improve the transmitter as well as the receiver. The pure unmodulated wave which we will have soon will be better for frequency measurements.

General Ferrié. — General Ferrié thinks it would be better to wait until the meeting next year when we would be better aware of the measurements involved, and then make a decision on this question.

Dr. Dye. — In the meantine I presume it will be desirable that some preliminary experiments be made to this end. I think it may probably be left in that way for the present. With the understanding that at the next general meeting we shall have considerably more information with respect to the standard frequencies.

Dr. Goldschmidt. — Thinks that subcommittee would be useful in order to see what work must be done during the next year and coordinate the different work of the different laboratories. The subcommittee would make a plan of the different things to go into the question so that we can be prepared next year to take definite position.

Dr. Dye. — Is it the view of the committee to form a small subcommittee for this purpose?

Dr. Dellinger. — We ought to have Dr. Taylor's advice on that. It is his service that would need to make the experiments and we should ask him if they feel that an international committee would be of value.

Dr. Taylor. — We just want to make ourselves useful if we can help out. It is to be supposed that if we carry out such attempts at transmission on very accurately controlled frequencies that it would be our Bureau of Standards here, the official organization in charge of frequency standardization in this country, who would cooperate with us in saying what that frequency is. I think that goes without saying as far as this country is concerned. What the rest of you would do I do not know but our idea is that we have these transmitters, they are quite good as they stand and they can be made much better, if you want these constant frequencies. Now if we give out these transmissions and they are simultaneously measured by our Bureau of Standards, by the British Laboratories, and by the Physikalisch Technische Reichsanstalt, etc., we ought to get some interesting information, and so I do not think that I ought to play a very prominent part in the committee. I am just giving you some tools and you go ahead and use them. Dr. Dellinger, I think, is the man who is primarily interested in this.

Dr. Appleton. — It seems to me that a committee could discuss the kind of preliminary experiments that could be tried and I would propose that the committee consist of five people only : Dr. Taylor, Mr. Minohara, Mr. Mesny, Dr. Dellinger, and theChairman, Dr. Dye.

Dr. Dellinger. — In that connection I would like to ask whether the Japanese representatives contemplate that measurements might be made in Japan during this year of preparation.

Mr. Minohara. — I am not certain. Our specialists have made experiments on frequency, and they prepared a report about it which was given to the members of this meeting. They are continuing their measurements and research, the results of which are not yet complete, but probably next year they shall have complete results.

Dr. Dye. — It is agreed that the committee as proposed by Dr. Appleton be formed for this purpose.

Dr. Dellinger. — I am not entirely clear whether the period of trial shall be considered as including measurements on Bordeaux as well as American stations. As General Ferrié said it was only necessary to install the controlling apparatus at Bordeaux I should think that any measurements would include measurements on that station too.

Dr. Dye. — I understand that measurements should be made on all stations which are considered suitable for the purpose, and any program drawn up would incorporate any station considered of sufficient power.

We shall now turn to a discussion of piezoelectric standards.

Dr. Dellinger. — Most of the research on piezoelectric applications in the last year or two which has been published has had to do with the piezo oscillator. Piezo resonators were formerly investigated rather thoroughly, and there are no doubt useful possibilities in the resonators. They have certain applications to which they are more suited than the oscillators. I would like to ask if any of those present have been studying quartz plates with the use of resonators in view, and if we could have a little information on their present possibilities.

Dr. Dye. — Fortunately we have Prof. Cady with us today, and I would like to ask him to start the discussion on this subject.

Prof. Cady. — I would like to thank the Commission for the privilege of being here. With regard to the question of quartz resonators they have occupied more of my time than the oscillators. I agree with what Dr. Dye and Dr. Dellinger have said concerning the possibilities and conveniences in the use of resonators; particularly I have been struck with the freedom from undesired frequencies on the part of the resonators. When a crystal is used as a resonator it is quite possible by putting it into an absorbing circuit to observe an absorption of power on the part of the crystal which is extremely sharp as to resonance so that by having a generating circuit that is under very precise control it is possible to work up and down, so to speak, on one side of the curve of resonance and in that way to make frequency comparisons which are quite extraordinarily precise. The peak of the resonance curve or rather the tip of the crevasse can be determined with a precision which I think is quite as great as that of the quartz crystal as an oscillator. There are other practical advantages, and in particular the one which Dr. Dye has stressed, namely that the constants of the electric circuit have a less disturbing effect upon the critical frequency of the crystal than is the case when the crystal is being used as an oscillator.

Dr. Mesny. — These questions are illuminated by the researches made by Mr. Tawill in France, to which General Ferrié alluded in our previous session. Mr. Tawill's experiments were so interesting that I have brought his apparatus with me, and have set it up at the Bureau of Standards where I will be glad to demonstrate it to anyone interested.

On produit de la lumière polarisée linéairement, et on l'éteint avec un nichol. Si sur le trajet de cette lumière on interpose une lame de quartz dont l'axe optique est normale à la direction des rayons lumineux, la lumière réapparaît; mais on peut obtenir à nouveau une extinction en introduisant une seconde lame de même épaisseur que la première et convenablement orientée.

Si alors on excite l'une de ces lames au moyen d'un oscillateur, elle entre en vibration et quand il y a résonance les vibrations mécaniques donnent lieu à des déformations qui modifient les propriétés optiques du quartz en différentes régions. La lumière réapparaît en ces régions et des figures caractéristiques de la fréquence se décrivent; en photographiant ces figures on a le moyen de retrouver les fréquences correspondantes avec une grande précision.

Dr. Dye. — I have seen this demonstration as set up by Prof. Mesny at the Bureau of Standards. It shows in a very simple and elegant manner the mode of vibration of piezoelectric resonators of rod or plate form.

The method consists in viewing the resonator through a Nicol prism using a beam of polarized light. The bar or plate must of course have the appropriate faces parallel planes and polished.

A small plate of natural quartz similarly cut and of equal thickness is placed in a reversed sence in front of the piece under examination. The adjustments are then made by rotation of the compensating plate and of the analyzing Nicol — so that with the resonator at rest no light passes.

When the resonator is set into forced resonant vibration in any of its modes by the application of a smoothly and finely variable alternating electric field at the appropriate frequency, the portions of the resonator which are regions of considerable tensible or compressive stress become bright. The plate or bar becomes divided up into regions which are alternately bright and dark corresponding to nodes and loops of stress.

On plates figures similar to the well-known Chladri figures are very beautifully reproduced. This confirms the opinion now gradually gaining ground that plates vibrate either by overtone nodes of longitudinal kind or by flexure in nodes and loops rather than by plane compression and tension across their thickness. The patterns are sometimes regular with radial symmetry and sometimes are irregular with local peculiarities in the vibration form.

The method appears to be of considerable value in analyzing the vibrations in piezoelectric crystals and also possesses the advantage that photographs may be readily taken giving permanent records of the modes of vibration.

Prof. Vanni. — Experiments with piezo oscillators have been made in the laboratory of Prof. Vallauri at the Marine Laboratory at Livorno. With them he has obtained results in close agreement with other countries. Frequencies may be measured to a precision of several parts in 10,000 or perhaps one part in 10,000.

Dr. Goldschmidt. — It would be interesting to organize a means to furnish quartz crystals to different laboratories because actually it is difficult to procure them. I wish to suggest the appointment of a subcommittee to study the possibility of procuring quartz crystals for distribution from some central place without their being too expensive. The subcommittee might try to make arrangements necessary to exchange the different quartz plates as the Japanese delegate proposed.

Dr. Wheeler. — As to the exchange of quartz crystals, I think it is rather important that it be understood that it is necessary to transport not only the crystal but also the tube and the associated circuits. The whole thing must be exchanged if great accuracy is to be obtained.

Dr. Dellinger. — While there might perhaps be some advantages in favor of the suggestion of Dr. Goldschmidt, a few thoughts occur to me unfavorable to that proposal. While the appointment of a subcommittee of the Commission on Measurements for a piece of work which has actual international aspects is appropriate and would be helpful, it seems to me rather doubtful whether a subcommittee would find anything useful to do in this particular case. As far as the securing of a supply of quartz plates goes, that verges into a commercial question. There are places where they can readily be purchased. There is also plenty of information available which will enable anyone to make up piezo oscillators. As for exchanging standards, there are comparisons already in progress between the different countries by this means, so it does not appeal to me that this particular thing would be one where a subcommittee of this Commission would find anything to do.

Dr. Goldschmidt. — The furnishing of quartz plates from a central office is not a commercial thing; it is a practical way to do international work. I speak of this because it must help international work. I am quite sure that some time amateurs will work also with crystals and will need much information. It is our duty to try to give out this information. We have received numerous letters from different people who ask us that thing. It is the same idea but from a different point of view as the Japanese proposal.

Dr. Taylor. — Don't you think that the Bureau of Standards and other national laboratories could be entrusted with this duty of intercomparing standards? I do not think there is any serious difficulty in obtaining quartz and in obtaining different circuits for the apparatus they are used in. Some of our circuits and those of Dr. Dellinger have been published in the Proceedings of the Institute of Radio Engineers. You can have no difficulty in setting up the quartz in the circuit, but it seems to me that should be a matter of official arrangements between the different national laboratories. When it is done it should be official in order that the art should get full benefit.

Dr. Appleton. — Then the amateur would apply to his own national Bureau of Standards.

Dr. Goldschmidt. — That is not a very practical way. I would ask if it is possible to find a more practical way to distribute quartz plates. For instance, the Bureau of Standards, or someone else, would say, « I will supply all the world, » so in that way it would make it very cheap. There are numerous people who write to me and to the Union asking if it is possible to arrange this so they can find crystals which are not too expensive and yet good, so I think it is necessary that we think how this can be done. Is it not possible that one laboratory could do this, in America, in England, or somewhere else?

Dr. Taylor. — It is necessary to have the complete apparatus and not only the crystal.

Dr. Cady. — I have no opinion to offer on the advisability of a subcommitte for securing crystals more conveniently but it does occur to me that possibly if such a committee were created its function might be somewhat extended, for example, it would be desirable to have the subcommittee gather together all information on the best method of cutting and mounting the crystals and associated circuits. Various amateurs have made quite useful contributions to the art and such a committee might busy itself in getting together all possible information. It might perhaps make suggestions to various national laboratories or other laboratories of specific investigations that it is desirable to carry out, for example, under certain circumstances, should we not get better results by the oscillator, under what conditions the crystals would be best mounted for extreme constancy. There are difference of opinions as to the best practice in mounting crystals. I can see that there might be various questions that such a subcommittee could take up together and which would further the progress of the work.

Dr. Goldschmidt. — It would be a first step in the realization of that work, after which it would be possible to find a way to supply the different amateurs and laboratories with that apparatus internationally.

Prof. Vanni. — There are two questions here involved although they are correlated to some extent. The first question is the determination of a suitable standard of frequency, and the second question is the best method of utilizing such standards of frequency for the intercomparison of apparatus in different countries. In regard to the first question it is generally admitted that quartz standards are not only probably the most precise but also perhaps the most simple fundamental standards yet discovered. A great deal of work has been done for the past half a dozen years in different countries around the world upon these quartz standards and the results have been published. The United States Bureau of Standards, the National Physical Laboratory in England, the Military Laboratory in France, the Reichsanstalt in Germany, the Marine Laboratory at Livorno, Italy, and numerous individual experimenters have had experience and they know how to obtain quartz samples, and there are also in these countries technicians in the cutting of quartz. It is a simple matter for this information to be correlated for the amateurs.

Dr. Goldschmidt. — Excuse me for insisting. I have received a great many letters in the past few months, people asking how to get those crystals. It is not absolutely necessary to have very exact crystals, but it is necessary to be able to obtain them, not only the amateurs but also the people of the different laboratories who ask us that. They say, « If we buy them it costs us a lot of money and it takes a lot of time to have them standardized. Can we not get crystals of a known accuracy immediately and cheap? » So that is why I ask you this. Would it be possible that you give me an opportunity later to make a motion to supply from an international place crystals which would be sent at a price very cheap to amateurs and others? It may not be the work of a subcommittee, but I would ask you to agree with the principle of the idea to help international work.

Dr. Dellinger. — The idea of having a subcommittee to secure and disseminate information that would be useful to persons who are required to make frequency measurements, I think we should all support. I think furthermore that we should do everything possible to support the ideas of our General Secretary in the plans he is formulating for making the Union more useful. As a start toward the compiling of information that could be distributed to interested persons, I submit herewith a bibliography of piezoelectric properties, oscillators, and resonators (1). In my previous statement against one feature of Dr. Goldschmidt's proposition I said that the furnishing of standards as he proposed would be a commercial matter. I meant simply to indicate that when you furnish something physical to somebody it necessarily involves a financial consideration and must become a commercial matter. If we set up an arrangement to furnish quartz plates or any other forms of frequency standards we are competing in a commercial business. I use the term « commercial » because in the United States we have gotten accustomed to thinking of it simply as a commercial matter; those who wish to obtain such instruments in this country can purchase them. As to the Secretary General's direct question whether we cannot help him to satisfy these people by furnishing quartz crystals to them, I am sorry to say the answer must be no. It is impossible to obtain a satisfactory standard so easily. A quartz plate alone will not suffice; it must be an elaborate instrument. To publish information on piezo oscillator construction, etc., which would enable people to secure fairly cheap and pretty accurate standards, suitable for most purposes, that is easy. That could be done and a committee might be helpful for that purpose. But when it comes to the actual furnishing of standards, I think that is not the function of this Union. That is a Governmental function regulated by law in the different countries.

Dr. Appleton. — Do publications of the Bureau of Standards include instructions on how to make good quartz plates from the quartz crystal.

Dr. Dellinger. — No, but there are articles on the subject in the list I just submitted. Many American amateurs have secured quartz crystals, ground the quartz plates themselves, and constructed circuits, and thus secured a pretty fair standard. Of course the setting of the frequency value for that standard they do not do themselves. That must be done in each country in accordance with the national laws.

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(1) The bibliography appears as an Appendix to the minutes of this meeting.

Dr. Taylor. — I support Dr. Dellinger most emphatically in this matter. As far as this country is concerned I do not think that the Executive Committee of the American Section of the International Union could possibly support any proposition which looked to the furnishing of actual material because it would really involve going into business and buying material for apparatus and then selling it again. One of the fundamental principles of American Government is not to interfere with recognized interests and manufacturers. I can say most emphatically as far as the Department with which I am connected is concerned that we could not have anything to do with it. If some foreign Government laboratory should request that we build them a frequency standard similar to the one which you have seen on the table here, that could be handled; but to supply even our own amateurs with such materials, to say nothing of foreign amateurs, is quite out of the question, and I think Dr. Dellinger will tell you that the Bureau of Standards could not legally enter into any such business either. Further we have well organized commercial concerns in this country who are manufacturing a fairly satisfactory secondary standard which would meet the order of precision Dr. Goldschmidt has in mind. In view of that fact, our attitude would be that such things are already available to the amateur. He buys his quartz, he grinds his crystal down, and he listens to it and compares it with high frequency stations and known frequencies, and he knows where he is, in other words, he calibrates his frequency meter on known stations. I think that you gentlemen from foreign nations will find that this matter will take care of itself in a little while. They will study a little on the technique of quartz, they will make their own crystals, and they will find more and more transmissions on definite frequencies and will get the comparisons. I fully appreciate Dr. Goldschmidt's desire to make this institution more useful but I am afraid we are treading on dangerous ground if we attempt to furnish quartz crystals to the amateurs.

Dr. Goldschmidt. — You are absolutely right, but I will try to find a plan to distribute crystals gratis to the amateurs. My work is to give satisfaction to those who write me. All the amateurs, if they had tools, would work better with us.

Dr. Taylor. — We could give out plenty of information, and I think what you could do is to interest some concern which manufactures small apparatus and instruments of precision in supplying to your amateurs these quartz crystals. That is the way it has been done in this country. Different manufactures have interested themselves in supplying our amateurs with quartz crystals. It is an easy step. If you can convince your instrument makers that it can be done with a moderate profit the problem is solved. The idea of correlating this information is excellent. We would be very glad to do what we can to supply information on circuits and a great deal of that has already been published and it could be collected and issued in some form.

Dr. Dye. — I would like to endorse exactly what has been said by Dr. Dellinger and Dr. Taylor. It would be quite impossible for us at the National Physical Laboratories to provide actual pieces of quartz. It would be against the law. Furthermore there are in England several firms who manufacture crystals comparatively cheaply of the kind quite suitable for the purpose now in view, and I imagine that it would not be at all difficult to compile a list of the firms from which such crystals can be obtained. Some of them provide certificates of sufficient accuracy for the purpose without the expense of sending them to a national laboratory, but those amateurs who want higher accuracy can send them to the National Physical Laboratory so I do not think there is really any difficulty at all in satisfying the wants of the amateur.

Dr. Hulburt. — If the amateur cannot get crystals, inside of a year or two, he will make a piece of apparatus that will work much better without using any crystal.

Meeting adjourned at 4 : 10 P. M.

APPENDIX. OCTOBER 18 SESSION.

Piezo Resonators and Piezo Oscillators.

W. G. Cady, Piezoelectric resonators. (Physical Review, **17**, p. 531; April, 1921.)

W. G. Cady, New methods for maintaining constant frequency in high frequency circuits. (Radio News, **3**, p. 392; Nov., 1921.)

W. G. Cady, A piezoelectric method for generanting electric oscillations of constant frequency. (Physical Review, **19**, pp. 381-382; April 1922.)

W. G. Cady, The piezoelectric resonator. (Proc. I. R. E., **10**, pp. 83-114; April, 1922.)

G. W. Pierce, Piezoelectric crystal resonators and crystal oscillators applied to the precision of calibration of wavemeters. (Proc. American Academy of Arts and Sciences, **59**, pp. 81-106; Oct., 1923.)

R. G. Harris, Oscillating crystals for wavemeter calibration. (Radio News, **5**, p. 1418; 1924.)

H. S. Shaw, Oscillating crystals (use in transmitting sets). (QST, **7**, pp. 30-35; July, 1924.)

W. G. Cady, An international comparison of radio wavelength standards by means of piezoelectric resonators. (Proc. I. R. E., **12**, pp. 805-816; Dec., 1924.)

W. G. Cady, The quartz crystal as a new wavelength standard. (Popular Radio, **7**, pp. 357-365; April, 1925.)

W. G. Cady, Piezoelectric standards of high frequency. (Journal Optical Society of America, **10**, p. 475; 1925.)

F. E. Nancarrow, Quartz crystal resonators. (Post Office Elec. Jnl., [London], **18**, pp. 168-174; July, 1925.)

D. B. Mc Gown, The quartz crystal oscillator. (Radio [San Francisco], **7**, pp. 29-31; July, 1925.)

C. W. Goyder, The piezo electric effect and its application to wireless. (Experimental Wireless [London], **3**, pp. 94-100; Feb., pp. 165-171; March, 1926.)

A. Hund, Uses and possibilities of piezoelectric oscillators. (Proc. I. R. E., **14**, pp. 447-469: August, 1926.)

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Note added Jan. 25, 1928. — A much more complete bibliography on piezoelectric has been prepared by Professor W. G. Cady and will be published in Proceedings of the Institute of Radio Engineers early in 1928.

Meeting of October 24, 1927

Dr. DYE, Chairman.

Dr. Dye. — I do not think we can give much time to many fundamental quantities involved in radio frequency measurements but they deserve a word in passing.

In addition to frequency, we are concerned with inductance, capacity, effective resistance, current, and voltage, as fundamental quantities. For most purposes, inductance and capacity can easily be measured to the accuracy needed in radio measurements. The basis of reference is usually a self or mutual inductance of value calculable from dimensions, or the use of bridge methods wherein capacity is measured in terms of resistance and frequency (Maxwell commutator method), and inductance is measured in terms either of capacity and frequency (resonance methods) or of capacity and inductance (various bridges).

These fundamental standards are usually of larger values, and are determined at lower frequencies, than concern us in radio measurements The stepping off in magnitude from the standard by the use of various bridges, and the linking on from low to radio frequencies, introduce uncertainties which at moderate radio frequencies do not in general become greater than one or two parts in a thousand for normal values of inductance and capacity. For small values of inductance, however, and for large and small values of capacity, the uncertainties become greater and these uncertainties are much augmented at high radio frequencies.

The internal inductances of condensers, the distributed capacities of coils to themselves and to earth, together with changes in the true inductance due to eddy currents in the conductors, render a separation of these various elements in an oscillatory circuit difficult at high radio frequencies. It is not very easy, therefore, to establish a basis whereby the true capacity and true inductance of a condenser or the true inductance and the effective self capacity of a coil may be measured at very high frequencies.

A comprehensive series of measurements on various combinations of inductance and capacity together with the use of loops of calculable high frequency inductance and condensers of assumed true high frequency capacity become necessary before we are enabled to effect a separation of inductance and capacity in a circuit such as is used at very high frequencies. This is a subject requiring some further attention with possibly an interchange of coils and condensers internationally. In this connection I might mention that I have arranged with Dr. Curtis of the Bureau of Standards to intercompare standards of inductance and capacity with those of the National Physical Laboratory. If any members of this Commission consider that such an intercomparison would be of value, I think it would not be difficult to arrange this for other countries. The comparisons referred to are of standards of relatively large values such as are used at telephonic and low frequencies but the extension of the work to include standards suitable for use at radio frequencies might well be considered. The designs of condensers and coils suitable for wavemeter and general standardizing purposes is of special importance.

CURRENT AND VOLTAGE. — The measurement of current and voltage is in a similar position to that of inductance and capacity. At moderate radio frequencies the accuracy meets most requirements. In many methods of measurements of various quantities, the valve voltmeter, as it is known in Great Britain, has shown itself to be very useful and sufficiently accurate for use over a large range of frequencies. Such a device can, of course, be adapted to the measurement of current.

At very high frequencies, however, the measurement of current and voltage presents great difficulties. These are augmented by the large changes in current from point to point in a circuit. The attachment of any device with leads is almost certain to introduce errors. For this reason certain kinds of hot-wire current indicating devices have been developed in which the simplest form is obtained, the mirror or other indicating device being directly attached to the hot wire.

The difficulties exist both at the transmitting end in the measurement of large currents and at the reception end in connection with measurement or the production of small voltages used for calibrating purposes.

In connection with the measurement of large currents at very high frequencies, I may mention that I have made some experiments on subdividing, using a large and a small fixed condenser in parallel, the idea being to measure the large current throught the total capacity in terms of the more easily measured small current through the small condenser. The experiments show promise but at frequencies of 15,000 kilocycles per second and upwards the internal inductances of the condensers and of the leads connecting them introduce large and rapid variations in ratio. It is possible to construct fixed condensers with much smaller inductance than those I used. It is itended to compare the method with current transformer method which so far as I am aware has not been proved for very high radio frequencies.

Effective Resistance. — The work which has been carried out in connection with this branch of radio frequency measurements during recent years has been so very extensive and varied that it is quite impossible to refer to it in any detail. It will suffice to say that, in addition to the well known older forms of resistance and reactance variation methods, we have new adaptations of these, new thermal methods, methods involving the reaction of the circuit under test upon the reactance of another circuit, methods involving critical conditions in valve circuits (these methods are claimed to be of great sensitivity), methods of balance and bridge methods have also been devised. A method using the integrated voltage squared of the damped oscillation which takes place when a charged condenser is connected to an inductance has also been developed by Petersen and has shown quite good promise.

All these methods and possibly others not included probably give satisfactory results when used with experience but here again an organized series of comparisons of the methods would yield valuable confirmation. An interchange of coils and condenser to be measured for effective resistance by different laboratories would also serve a similar purpose.

A large number of methods of effective resistance measurement refer to a whole circuit in terms of the current as some point in it. For high class work it is now recognized that even the best air condensers such as may be used in such measurements cannot be considered to have zero effective resistance. Work on the determination of the effective resistance of high class and other air condensers has been done during recent years in the United States. Germany, France and Great Britain. It seems fairly certain that in some cases a basis for the measurement of these losses in the best air condensers has been established so that other condensers used by various workers may be measured at a standardizing laboratory.

On the theoretical side notable advances have been made in the development of formulas for the calculation of the effective resistance of coils of given dimensions and type of winding. Some work on the experimental verification of such mathematical work has also been done and the agreement has been found to be fairly satisfactory. But amongst the most careful experimenters the feeling has existed that ignorance in the knowledge of the condenser losses may have accounted for some of the discrepancies observed when methods involving the measurement of the effective resistance of a complete oscillatory circuit have been used.

Our increased knowledge of condenser losses and the greater variety and increased sensitivity of the methods of effective resistance measurement should make possible an advance in the linking together of the experimental and theoretical work in this field. The remarks here presented regarding effective resistance measurement refer chiefly to low and medium radio frequencies (up to 2,000 kilocycles per second). At high and very high frequencies the difficulties are enormously increased. No attempts have been made at all as far as I am aware to prove the accuracy of measurement of either effective resistance or current at short wave lengths. The definitions of the quantities is not always easy so that we are not quite sure what it is we have measured when an observation is made.

I will conclude this section of the subject at this point in case the subject matter is of sufficient general interest to warrant a discussion which can now commence but if it will be thought of more value we can proceed to a consideration of matters relating to the measurement of field intensity which is I imagine the branch of radio measurements of next greatest interest to that of frequency.

These remarks have covered a fairly large field of activities which concern the more fundamental kind of measurement, and it seems to me that they are worthy of discussion.

Let us consider what is the best method which exists to measure this capacity. In the first place I think it is rather difficult to say exactly how much of the change which may be observed in the apparent self-capacity of a coil is due to a real change in capacity with frequency and self inductance with frequency. It is a matter of considerable difficulty to separate these two changes. Ordinary methods for the measurement of the self capacity used at the National Physical Laboratory, capacity of coils is the one in which a condenser is connected across the terminals of the coil and resonance is taken at various known settings and a straight line is plotted through the points connecting the reciprocal of the frequency secured with the condenser readings. In cases of stranded coil of thinly stranded wire the lines are very straight when considered for frequency not too near that of self resonance of the coil, then near the added capacity is only two or three times the self capacity of the coil the points still lie on a straight line but it becomes necessary to extrapolate it back and one cannot always assert that the line is straight, but at the National Physical Laboratory we have not been able to effect a separation at wave lengths approximating to the resonant frequency of the coil. The resonant frequency is a quantity which can be determined in itself but there is always a break between that resonant frequency and the resonant frequency of a calculable is added.

Prof. Vanni. — I have also used this method. With antennas we must consider many kinds of capacities, effective or static. In regulations we require stations to give the value of the antenna capacity. Wich capacity would be best to fix for that? Also, what is the best method to measure the capacity?

Dr. Dye. — I think it would be best to ask for the effective capacity, but because of the difficulties of measuring that will probably be beyond the ability of a large number of amateurs. I am not prepared to say what is the best method to measure the capacity, but I should think that the simple method would be sufficient for most amateurs, in which a variable self inductance is included at the foot of the antenna. I think that in many cases the inductance which is used in practice in an antenna is not very large compared with the distributed inductance of the antenna so that one is not quite certain that the value obtained when large inductance is inserted corresponds with conditions under which the antenna is afterwards used.

Prof. Vanni. — What are the best practical methods to make comparison between a wavemeter and a standard wavemeter, for example, a piezo oscillator?

Dr. Dye. — At the National Physical Laboratory wavemeters which are sent for test are always tested by the use of an auxiliary oscillator source and valve oscillator which is set to precise frequencies given by the standard wavemeter which has a large number of fixed points. It is not a crystal-controlled one. Each point determined on the wavemeter sent for calibration is of an absolutely known frequency. The method of observation is that which is normally provided on the instrument. I am not quite sure whether you wish to know how wavemeters which would be used by various people, not necessarily associated with standardizing work, are done, or whether you wish to know how a highly accurate wavemeter would be calibrated. The question of standardizing a standard wavemeter from the one or two or perhaps a dozen fixed frequencies given by a quartz oscillator is not such an easy matter, I imagine, in those laboratories which only have quartz oscillators providing them with a comparatively few and widely separated number of frequencies in certain regions, especially in the regions of low frequency. If one has a quartz oscillator giving a fundamental frequency of 100 kilocycle, then the next frequency available from it is 200 and there is a very wide band of frequencies which has to be covered with interpolation and I do not know how laboratories which are not provided with the better form of standard can give an accuracy of better than one part in 10,000 in that band.

Prof. Vanni. — Is it the only method to follow in that case?

Dr. Dye. — No, I think by the use of auxiliary valve generators and by the use of their harmonics one can always build in a number of secondary frequency points, but in spite of that when it becomes necessary to make measurement to an accuracy of one part in 10,000 of some frequency which is quite known and one has then perhaps to interpolate, there is some degree of uncertainty in the interpolation if one uses even a high class standard wavemeter where one has to depend on scale readings. I do not think it is easy to get an accuracy of this kind when one has to interpolate between two points fairly wide apart.

Prof. Vanni. — From a practical point of view, can we say that two wavemeters such as those in an ordinary laboratory are for a sufficient precision but not extreme, can we guarantee these to retain an accuracy of one part in 10,000?

Dr. Dye. — It has been our experience in connection with wavemeters which have been sent for calibration from time to time that very few of them indeed retain their calibration from one year to another to an accuracy of even one part in 1,000 but a high class instrument will retain its calibration to two parts in 1,000 if it is well designed and well made, but it has to be an expensively made wavemeter to preserve an accuracy of two parts in 1,000 from one year to another. A very great deal depends on the particular use to which the wavemeter is to be put and on the range desired. In the Army we have small portable wavemeters which cover a comparatively wide range but in these wavemeters inductance is the variable portion. These wavemeters will preserve an accuracy of nearly one part in 1,000 over a long time but it is impossible to get such wavemeters with a temperature coefficient which is small enough to meet the requirements of practice.

Prof. Mesny. — It would be interesting in this exchange of standards to go farther than exchanges of inductance and capacity, but also the exchange of complete circuits and to compare these from the standpoint of frequency.

Dr. Dye. — I think that is desirable, not only from the point of view of fixed electrical circuits but even with the quartz controlled oscillator circuits. We are not sure what frequency they have as a result of transportation. Experiments made at Bell Telephone Laboratories in which a quartz oscillator was set up and left running for many days and it is to get an idea of the kind of steadiness which can be obtained from them as an ultimate accuracy. If it were dismantled and removed to another room and set up again and conditions obtained, it does not follow that the same frequency would be obtained to the same degree of precision.

Prof. Mesny. — Have you any idea of the degree of precision obtained?

Dr. Dye. — I have in regard to quartz resonators which are very simple, and I think these are of a constancy certainly as great as two or three parts in 100,000 in a properly designed one, because I have received these for measurement. I have had the same one returned to the National Physical Laboratory several times; the owner returned it every year for recalibration and the measurements show that it had remained extraordinarily constant and it was very reproducible, but with electrical circuits the temperature effects are very serious if one requires an accuracy better than one part in 1,000.

Perhaps someone from the Bureau of Standards could tell us whether repeated measurements on the same quartz oscillator on different occasions has shown whether it was as accurate as it could be measured.

Mr. Hall. — We have found in our measurements at the Bureau of Standards on piezo oscillators that they check from one time to another very well. In fact that is one reason we undertook the intercomparison with the other laboratories by means of the two piezo oscillators which we sent abroad, and later by the temperature-controlled piezo oscillator. We have constructed several piezo oscillators without temperature control for the use of the United States Radio Inspectors, of the same type as was sent to the foreign laboratories. We have checked these from one time to another when they have been returned for recalibration and they have agreed very well indeed. I would say, the constancy was one or two parts in 10,000; they were considered to be very satisfactory for the purpose for which they were used. We have a great deal of such data which has given us considerable confidence in piezo oscillators; but as has been pointed out here and in other meetings, if they are to be used to a high degree of accuracy there are a great many details to be taken into account.

Prof. Mesny. — Have you any results about the comparison of wavemeters which have been recalibrated several times?

Mr. Hall. — It is seldom that we have the same wavemeter returned for calibration here. In fact, we do not do much of the commercial standardizing of wavemeters. Checking up on wavemeters in the laboratory (we have different types of them there, some of which we call our working standards and others which are less accurate), they may vary more than 1/10th percent easily in the summertime when we have high humidity. In fact they change from one day to another. A great deal depends on the coil design.

Dr. Dye. -- A good deal of the change which occurs in the calibration of wavemeters with humidity can be traced to the effective self capacity of the inductance coil because of the absorption of moisture by the insulation of the wire.

Prof. Mesny. — What is the percentage?

Mr. Hall. —- By using a large condenser, change from this cause is kept to a very small percentage. We avoid the low capacity settings on the condenser.

Dr. Dye. — In all wavemeters in which continuous variation of frequency may be obtained, for an accuracy of one part in 1,000 it is quite essential to have a large portion of the capacity not variable, so that your range using any one particular coil is only a narrow one. I think that can be taken as a certainty.

Prof. Vanni. — Dr. Dye spoke of field intensity measurements. As I had occasion to read a very interesting report which was published by Bown and Jensen on the measurement of field intensity, and which was published in the Proceedings of the Institute of Radio Engineers, and as it deals with a practical apparatus and also very interesting instrument, I would like to ask if anyone here has had occasion to work with that apparatus, which was used by the Radio Laboratory of the Bell Company.

Dr. Dye. — I do not know of anyone in England who has used that except perhaps some of the representatives of the Bell Telephone Laboratories there. I have not seen any results obtained by such measurements made by them, and I am not really directly connected with measurements of field intensity myself, so I cannot speak on that, but there may be others here who have used that apparatus.

T. Parkinson. — As to the general method the Bell Laboratories use, they have a method of measuring with a coil antenna, receiving a signal from a station with the coil pointed toward the station and then turning the coil so as to cut out this signal and introducing an equivalent signal from a local oscillator, the current being passed through a one-ohm resistor in the center of the coil. This current is measured and then reduced by an attenuation system. The ratio of attenuation is known. The signal from the local oscillator is adjusted to give the same deflection on the output galvanometer of the receiving apparatus as produced by the signal from the station. In that way, knowing the dimensions of the coil antenna and the voltage across the one ohm, it is possible to calculate the locally introduced voltage, which was adjusted to be of the same value as the received signal.

Dr. Austin. — Mr. Pickard was the first to try that sort of measurement. He had a somewhat different attenuation system. The Western Electric Company uses an attenuation box which they had originally designed for their telephone work. The accuracy of the method, I think, depends very much on the accuracy of that attenuation and its independence of the frequency within the ranges which are desired.

Dr. Dye. — I think the question of the measurement of field intensity will have to be divided into a large number of questions particularly when comparing one complete outfit in one country with that in another. They differ at almost every point, and it will have to be divided up into subsections. It seems very difficult to form any kind of basis on which to discuss the matter or even to compare setups; on this Dr. Austin, of course, is a great authority.

Dr. Austin. — The problem is not very difficult on very long waves. The difficulty seems to come when you get below 100 meters. It of course depends very much on your ability to measure the degree of attenuation which you give to your current in your artificial signal. I think the other factors are comparatively simple until you come to quite short waves, except, of course, the matter of the shielding. That is a matter of great importance but is fairly well understood now, I think, so I do not imagine there ought to be a great deal of error from that source.

Dr. Dye. — There were a number of comparative measurements made, I believe, at the Bureau of Standards between different field strength measuring sets for long waves. I understand the results obtained were considered fairly satisfactory. They all involved an aural method of observation, not visual.

Dr. Austin. — Both aural and visual were used. For the stronger signals you could use the visual method. There are so many errors, however, of other kinds, that if the aural method is used carefully I do not think there is a great deal of difference between the two except during strong atmospherics.

Dr. Dye. — I think the exception as to strong atmospherics attains some importance in England, and very considerable attention has been given to the development of methods which are visual and which are intended to give more accurate results under varied conditions.

Dr. Austin. — Where the signals are weak and where you have a great deal of trouble from atmospheric disturbances, is the aural method considered superior?

Dr. Dye. — In some methods where the actual keying of a signal can be seen it is quite easy to discriminate between atmospherics and signals.

Dr. Austin. — With continuous atmospherics you are not able to do that. It is only where you are dealing with separate crashes and that is the kind we do not have in this part of the world.

Prof. Mesny. — Have you made comparisons with other methods in your laboratory?

Dr. Austin. — We participated in a comparison of our apparatus with the apparatus of the Bell Telephone Company and that of the Radio Corporation of America. The Radio Corporation of America uses a mutual inductance type of attenuation and the Bell Company uses the attenuation net, resistance type, and we got very good agreement on the strong signals. When it came to heavy atmospheric disturbances they did not always agree very well, but evidently it was not the fault of the method of our comparisons but more from the difficulty of getting observations through the heavy atmospherics. I should say that on strong signals the accuracy was, roughly, 10 %, and as the signals became weaker the accuracy decreases so that for very weak signals with atmospherics there might be 30 % or even greater difference.

Our present method of measuring during atmospherics has seemed to be very satisfactory. We have tried various schemes. Of course, to begin with, we are at something of a disadvantage compared with the methods in which the artificial signal is introduced regularly with the atmospherics, but by producing an artificial signal of the same intensity apparently as the signal which we are hearing from a distance and then measuring that first with the atmospherics and then on an artificial antenna free from atmospherics we get a ratio of correction which we think is fairly satisfactory. There are some disadvantages in using the other method in which the loop is swung around at right angles. You get the atmospherics, of course, in both cases, but there is an interval of time in throwing the loop from the direction pointing towards the station to the direction at right angles which makes it considerably more difficult to make an estimate by ear of the strength of the signal. It would be better to have two loops, one pointing toward the station and the other in a position where you could not hear the station, and that would probably work very well.

Prof. Mesny. — There would be discrepancies between the two loops?

Dr. Austin. — With sufficient care, it could be done, I think. We used the method of audio-frequency comparison mainly because it is so convenient for observing a large number of stations one after the other. With our method there is only the tuning of the ordinary circuits that you have to use for tuning in the stations. while with the radio frequency method you have to also make all the adjustments on the radio frequency comparison set as well as on the regular receiving set.

Dr. Dye. — It would be very desirable to have intercomparisons of the methods of producing the attenuation, I think, especially for the shorter waves. I think the difficulties are small at the longer wave lengths (greater than 2,000 meters), but at the higher frequencies methods of attenuation are open to criticism.

Dr. Austin. — I am not sure that there have been any such comparisons. There may have been comparisons between the Radio Corporation system and the Bell Telephone system, but I have not had any information in regard to that. I do not know quite what the Radio Corporation is using for short-wave measurements.

Mr. Parkinson. — They have used the mutual inductance method, and now they are said to be using practically the Bell method. They do not rely on the calculations from the coil antenna but calibrate the whole outfit from transmissions from a condenser antenna. Thus they are not calculating by the usual method of either the Bell system or the Radio Corporation system.

Dr. Austin. — Of course there is the possibility of error due to the ground effect of the condenser antenna. We do not seem to make much progress in regard to getting any high degree of accuracy.

Mr. Parkinson. — The Bureau of Standards is trying out a method a little different from either of those, yet practically the same. It is a method in which we introduce a voltage across one ohm in the center of the coil antenna without attenuation, and then attenuate by means of a voltage divider across the primary of the first intermediate frequency transformer of a superheterodyne receiving set. The intermediate frequency being always the same, any errors in the voltage divider which might be due to changes in frequency are eliminated. Consequently we can match, with a small unattenuated signal introduced from our local oscillator, a larger or smaller signal from the station itself by utilizing the known ratio between the two amplification settings employed. We get rid of some of the difficulties due to forcing the oscillator. How wide are the variations on short distance transmissions, Mr. Kirby?

Mr. Kirby. — We have been making some tests using a condenser antenna as transmitter, the condenser plates being placed 15 centimeters apart. The distance from the transmitter to the receiver varied from 500 to about 1,500 meters; we used different distances. Our results for the most part checked within 10 % the values given by transmission formula, a great many of them better than that. We still have some trouble, mostly due to transmitter and ground effects, which cause the remaining error.

Dr. Austin. — Would your variations at different distances check up?

Mr. Kirby. — There is not much difference with distance.

Dr. Austin. — Did you find any trouble from an apparent change in ground conditions, as you test in different directions or over different distances in the same direction?

Mr. Kirby. — Ground conditions were almost uniform. We set our receiving apparatus on the river wall and the transmitting apparatus on the river wall on the other side, and simply moved the receivers along the river wall.

Dr. Austin. — You did have trouble over land?

Mr. Kirby. — There were slightly more variations in our readings over land.

Dr. Austin. — Did they check up with what you get now?

Mr. Kirby. — Not quite so well, no. The land was uneven and there was a creek between transmitter and receiver.

Dr. Austin. — What wave length were you working on?

Mr. Kirby. — We have been working on frequencies from 600 to 875 kilocycles.

Dr. Austin. — Did you undertake to determine your absolute accuracy, or only the ratio of observations which you could repeat?

Mr. Parkinson. — We used several means of checking our absolute values, that is, checking with the theoretical value across the river. The values measured by our intensity apparatus checked within approximately 10 % of what the transmission formula would give; and we used the distance between the plates for the height of our antenna. By not relying on a single method, we are able to have far greater confidence in the absolute values.

Dr. Dye. — I do not think there should be any great difficulty in comparing methods by which calibration is made if it does not involve the use of a station for that purpose, and some useful work could be done in comparing attenuation systems in use at the present time.

Dr. Austin. — The best check on accuracy of these methods is of course to try several different methods and if they agree the chances will be pretty good they are correct.

Dr. Dye. — The question of the sending station can be entirely ignored because at present one uses as a basis the product of effective height and the current whereas presumably one should use the intensity at some point near the transmitter, and it seems to me that it would be better if the definition of the power of the station could be made in terms of the radiated energy in some way directly.

Dr. Austin. — Usually the effective height of the antenna is determined by a nearby signal measurement so that it amounts to about the same thing, if the effective height is really measured. It is almost impossible to calculate the effective height of any antenna with accuracy.

Dr. Dye. — Before you know enough about the intensity of a station you want to know it in some particular direction and the effective height, if it has been calculated back, does not tell you what you want to know.

Does Prof. Vanni have any remarks to make about the radiated energy from antennas?

Prof. Vanni. — Je vais dire quelque chose à ce sujet, en exposant le résultat de quelques études qui ont été faites en Italie par le colonel Sacco of the Italian Army. He has designed some differential condensers which are very useful when you want to separate stations working on very high frequencies. A summary of his work follows :

1. *Condensateur différentiel.* — Résumé : En étudiant un certain type de condensateur destiné à obtenir une variation très petite de capacité entre des limites données, M. Sacco démontre ce principe fondamental : « Par une construction appropriée de l'étendue angulaire et du profil des armatures fixe et mobile d'un condensateur et en limitant le déplacement de celle-ci à la différence des ouvertures angulaires des deux armatures, on peut rendre aussi petite qu'on veut, la variation de capacité qui correspond à l'unité de surface commune aux deux armatures (Principe différentiel de la construction des condensateurs). »

En introduisant certains paramètres appropriés, M. Sacco obtient l'équation générale des condensateurs différentiels qu'il étudie et résout, soit analytiquement, utilisant les propriétés des nombres de Bernouilli, soit par voie graphique de manière à obtenir le tracé graphique du profil de l'armature mobile. Il fait voir que la théorie du condensateur ordinaire peut être considérée comme un cas particulier de celle du condensateur différentiel, et qu'on est ainsi conduit à construire trois types de ce condensateur :

a) A variation uniforme de fréquence (V. U. f.);

b) A variation uniforme de longueur d'onde (V. U. λ);

c) A variation uniforme de capacité (V. U. C.).

Il donne des exemples pratiques et numériques de ces trois types en montrant qu'on peut par exemple dans le type (A) obtenir une succession continue de 100 ondes dans la bande de fréquence entre 10,000 à 11,000 kc/s, chaque onde étant séparée de l'autre de 10 kc/s, et dans le type (B) on peut obtenir aussi une variation continue de 100 ondes dans la bande $\lambda = 30$ m. et $\lambda' = 27$m.3 chaque onde étant séparée de la suivante par 2cm.7 de longueur.

2. *Proposition d'une nouvelle unité de mesure radiotélégraphique* (force cymomotrice). — En partant de l'équation de Hertz qui donne le champ électrique F dans le cas d'un terrain plan bon conducteur et dans une atmosphère parfaitement diélectrique, M. Sacco fait voir qu'on arrive à la formule suivante de la force cymomotrice :

$$E_c = Fr = 120\,\pi \frac{hi}{\lambda} \text{ volts.}$$

qui donne, dans le système pratique, m. V. A., ce qu'on peut appeler la *force cymomotrice* de l'antenne, c'est-à-dire son efficacité à rayonner des ondes électromagnétiques, indépendamment de l'absorption de l'atmosphère et des autres causes d'atténuation.

Par des exemples numériques concrets, il montre comment on peut se servir de cette nouvelle grandeur pour caractériser, avec d'autres quantités bien connues (moment électromagnétique, puissance d'antenne, etc.) une émission donnée et il fait voir aussi comment on peut se rendre compte du fait démontré chaque jour par l'expérience, que deux antennes différentes travaillant, l'une sur onde longue et l'autre sur onde courte, peuvent très bien avoir, en conditions favorables, la même portée nocturne quoique ayant des moments électromagnétiques et des puissances d'émission, tout à fait différentes.

Exemple :

$i = 40$ A, $h = 100$ m, $\lambda = 12$ km, $hi = 4000$ mA,

$$E_c = 377 \frac{hi}{\lambda} = 126 \text{ volts.}$$

$i = 1$ A, $h = 10$ m, $\lambda = 30$ m, $hi = 10$ mA,

$$E_c = 377 \frac{hi}{\lambda} = 126 \text{ volts.}$$

3. *Sur l'induction mutuelle entre cadres radiotélégraphiques voisins.* — Etant donné l'im-

portance toujours croissante, que l'on donne aux cadres employés comme transmetteurs, il convient de pouvoir déterminer, à priori, les conditions dans lesquelles ils peuvent transmettre sans se gêner mutuellement, et, en particulier, de calculer le coefficient d'induction mutuelle en fonction de leur position réciproque, c'est-à-dire, de la distance des centres et des angles que la droite des centres fait avec leurs plans.

Une formule de ce genre a été donnée par M. Mesny dans son intéressant ouvrage : « Usage des cadres et radiogoniométrie. » M. Sacco considère le cas où le centre des cadres ne se trouve dans le même plan horizontal, et il obtient une formule qui donne, en particulier, les mêmes résultats de M. Mesny, et dont il se sert pour donner des règles très simples pour déterminer la position d'induction mutuelle nulle de deux ou trois stations à cadres. Les résultats du calcul ont été trouvés tout à fait conformes aux résultats que donne l'expérience.

Prof. Mesny. — One of the questions which Dr. Dye put forth at the beginning of this meeting was the question of measurements of intensity with very short waves. Trouble comes from the wires which come from the thermo-element to the galvanometer and also from the capacity of the galvanometer. I used an apparatus which was designed by Holweck. It consists chiefly of a very thin wire which is stretched inside of a vacuum tube. On the middle of that little wire is a spring and if you have a current passing through the wire, of course the end of the spring moves a little. If you look beside the end of the spring with a microscope you can see this movement and the sensibility of this apparatus was such that it was possible to see a movement of 15 divisions for 1 1/2 m. a. which was not a great sensitivity, but Holweck was to improve it. Each division was approximately 1 mm. in dimension seen at a normal distance of vision. Thus you see all the capacity and connection wires are eliminated. I give that information to this meeting because I think it may be helpful in some investigations.

Dr. Dye. — I am sure we are very much indebted to Prof. Vanni for the three short papers he has read here this afternoon. With regard to this, I think the design of condensers to fulfil these special requirements has already received considerable attention, particularly in England. We have had uniform frequency scale condensers there for a number of years. I am not sure but I think I was the first to use it in England on my standard wavemeter in which it was quite desirable to have equal angular spaces corresponding to equal distant frequencies. That was five years ago, but more recently I have seen the mathematical theory of that work out in one of the papers of the Proceedings of the Institute of Radio Engineers. Of course the uniform wave length scale condenser was introduced by Mr. Dudell and has been known for a number of years.

With regard to the second paper on the use of a new term, that sounds to me an extremely useful idea. As far as I understand it, this term would represent the intensity at unit distance from the antenna expressed in units of voltage. That of course resolves all the difficulties of different sizes of aerials entirely. It sounds very useful and I should like to have Dr. Austin's ideas on that. It would be necessary to introduce some word in English to express it. Prof. Kennelly could do this.

In regard to mutual inductance formulas, I take it the idea was that if you have a number of small transmitting stations quite near together they may cause interference between one another. That, of course, does not permit of much discussion, because mathematical formulas give you that. There are different processes by which it is done and from the agreement with Prof. Mesny's calculations, I imagine there is no possible doubt at all they must be quite accurate and trustworthy. I was also interested in the thermo-instrument Prof. Mesny has described, and I shall probably make one myself. I do not know what the effective resistance of an instrument which indicated 1 1/2 m. a. would be but I presume it would be many ohms, perhaps 50 or 60. That degree of sensitivity is by no means small.

Dr. Austin. — I do not quite understand the method of using that milliammeter you were speaking of.

Dr. Dye. — There is a fine platinum wire in a glass tube in a bulb and a small spring, one end being fixed against the midpoint of the wire and causes it to sag a little and when the wire is heated by the current it sags still further, and measurements are observed on the other end of the spring. The spring is merely to keep a convenient tension on the wire. You actually observe the sag of the wire by a microscope.

Dr. Austin, what are your ideas about the use of a new term for field intensity?

Dr. Austin. — I should want to think it over a little bit.

Dr. Dye. — Has anyone any further comments?

Dr. Mesny. — I should like to say something also about the measurements of the field intensity. When you measure field intensity you are obliged to use a local oscillator, and the chief difficulty comes from the necessity to measure some current or some potential difference in that local oscillator. As it is not easy to measure very weak currents it is necessary to have in that local oscillator a strong enough current. Then comes the difficulty of correctly shielding the local oscillator. This difficulty is increased when you work on very high frequencies. Then other difficulties come into the question, as difficulties of capacity, etc. I made some experiments, not measurements as yet, and I used for the local oscillator a frequency which was much lower than the working frequency. Then in the inductance coil by means of a tuned circuit I took a harmonic of the current of the local oscillator. I found during those experiments that for the same apparatus during two weeks, the ratio between the harmonics and the principal current remained the same. Thus it seems to be possible to make a calibration in the laboratory which permits determining the ratio of the harmonic current from the principal current; the latter is very easy to measure because it is much stronger No difficulty comes from the shielding because current of the frequency at which the receiver is sensitive is very weak, anyway. My experiments were also concerned with parasitic capacities; and as I told you the other day I had in my coupling box a means of controlling the parasitic electromotive force, because there is a position of the coil for which the coupling must be zero; when the coil is put in this position no electromotive force can be produced within. Also I used two coils so constructed as to produce a sinusoidal law of variation of the output electromotive force. During these brief experiments I found that the variation pretty well conforms to theory, showing that capacitive coupling is not very important. I have not made measurements with this system, but I expect to make some. I think that it is an interesting method of operation for a local oscillator.

Dr. Dye. — There would be considerable difficulties in measuring the ratio of the fundamental to the harmonic.

Dr. Mesny. — Not very much. In the laboratory it is possible to use very sensitive apparatus.

Dr. Austin. — You had a strong enough current so that the harmonic was still strong enough to measure?

Dr. Dye. — Your idea was that you did not need to have a large current at the high frequency. Not so much as it would have been at the oscillator upon giving out that high frequency.

Dr. Austin. — Was your harmonic current weak enough to be used for comparison purposes on weak signals if you could still measure it?

Dr. Mesny. — With the coils I used it was possible to have a variation of the ratio of the strongest electromotive force I can put in the coil and the weakest, which is a ratio of approximately 50,000. It is readily possible to make measurements with the strong electromotive force, and I assume that the ratio is the same for weak electromotive force. As I said, it is possible to have a sinusoidal law of variation of the coil control.

General Ferrié. — Are there any questions to take up which relate to the proposals of the British National Committee?

Dr. Dye. — The questions which we have been studying are those which were put forward by the British National Committee. It is certain that we cannot reach any final conclusions in these discussions. They could go on indefinitely. All the points which were proposed by the British National Committee have received discussion at these meetings.

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RECUEIL DES TRAVAUX
DE
l'Assemblée Générale tenue à Washington
en Octobre 1927

PAPERS
OF THE
General Assembly held in Washington
in October 1927

FASCICULE 4

Travaux de la Commission
de la Propagation des Ondes
(TEXTES ORIGINAUX)

Papers of the Commission
on Radio Wave Propagation
(ORIGINAL TEXTS)

PUBLIÉ PAR LE SECRÉTARIAT GÉNÉRAL DE L'U. R. S. I.
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Recueil des Travaux de l'Assemblée Générale tenue à Washington en Octobre 1927

Papers of the General Assembly held in Washington in October 1927

TRAVAUX DE LA COMMISSION DE LA PROPAGATION DES ONDES

(Textes originaux)

PAPERS OF THE COMMISSION ON RADIO WAVE PROPAGATION

(Original Texts)

La Commission de la Propagation des Ondes a été instituée par décision de l'Assemblée générale de l'U. R. S. I. tenue à Bruxelles en juillet 1922.

Les travaux effectués par les savants des différents Comités nationaux de l'U. R. S. I. qui composent cette Commission, ont fait l'objet de rapports et discussions à l'Assemblée générale de l'U. R. S. I. tenue à Washington en octobre 1927. On trouvera le texte de ces discussions dans ce fascicule 4.

La Commission de la Propagation des Ondes, présidée par le Dr. Austin, est composée des savants suivants :

The Commission on Radio Wave Propagation has been instituted by the decision of the General Assembly of the U. R. S. I. held in Brussels in July 1922.

The works made by the savants of the different National Committees of the U. R. S. I., who composed that Commission, have been summarized and discussed, during the General Assembly of the U. R. S. I. held in Washington in October 1927. This part 4 includes the discussions and reports.

The Commission on Radio Wave Propagation, the chairman of which is Dr. Austin, is composed of the following savants :

Belgique :	M. DE DONDER, Membre de l'Académie, Professeur à l'Université de Bruxelles. M. M. PHILIPPSON, Professeur à l'Université de Bruxelles.
France :	M. MESNY, Professeur principal d'hydrographie, Professeur à l'Ecole supérieure d'Electricité. M. PEROT, Professeur à l'Ecole Polytechnique.
Great Britain :	Prof. G. W. O. HOWE, University of Glasgow. Mr F. E. SMITH, C. B., F. R. S., Director of Research at the Admiralty. Mr J. Erskine MURRAY.
Italie :	Prof. A. ARTOM. Commandant J. PESSION.
Norvège :	M. H. PETERSEN, Ingénieur en chef à l'Administration des Télégraphes. M. T. ENGSET, Directeur général à l'Administration des Télégraphes. M. F. GYTHFELDT, Ingénieur à l'Administration des Télégraphes.
Portugal :	Capitaine de Corvette Eduardo Maria SOARES, Instructeur de Radiotélégraphie à l'Ecole de Torpilles et Electricité de la Marine.
United States :	Dr. L. W. AUSTIN, of the Bureau of Standards. Mr E. F. W. ALEXANDERSON, Chief Engineer Radio Corporation of America. Commandr. A. H. TAYLOR, of the Naval Research Laboratory.

Les séances de la Commission de la Propagation des Ondes, à Washington, étaient présidées par le Dr. *L. W. Austin*, du Bureau of Standards de Washington.

The meetings of the Commission on Radio Wave Propagation, in Washington, were presided at by Dr. *L. W. Austin* of the Bureau of Standards of Washington.

Meeting of October 11, 1927

Dr. AUSTIN, Chairman.

Dr. Austin. — I have prepared a report covering the work of this Commission in recent years. This is merely for discussion and does not carry any new information. Copies have been distributed, which I think you all have, so in going on with the discussions we shall follow roughly the order in which you find the various subjects in this report.

The work of the Commission has been very fruitful, I think. The original plans which were made in Brussels have not been carried out in any degree of strictness. Our first idea was that the office of the commission should act as a clearing house for the observations and information which was given by the different national committees. It has not been found possible to do this. The original plan was for the data to be sent in to a central office where it would be examined and put together and worked over and reissued. The way the work has actually come out is quite different. It has been found necessary for each of the National Committees to work up its own results and they have been communicated to the other National Committees when the various papers were completed. It, I suppose, is perhaps perfectly natural that this was the way that it had to be done, but it does not result in quite the kind of cooperation which was first planned. We have had excellent cooperation in regard to the furnishing of information regarding the data of the sending stations, etc., but we have not been able to get quite as close together as we had first thought.

It has seemed to me that in the future if we want to make the most of the possibilities of a commission of this kind that we ought to plan to have meetings of the commission more frequently than the meetings of the Union as a whole. Therefore, I would like to propose that we consider the matter of the possibility of meetings at least once a year at some point which would be the most central for the various countries interested; that would be, of course, Europe. It is going to be a matter of some difficulty for the countries at a great distance to come to meetings as frequently as we propose, but I think it would be worth while for as many of those as were able to get together at least once a year to talk over the problems because communication by letter is not equal to communication by word of mouth, and I think that we all must feel that if we could get together and talk over these problems at frequent intervals that we would progress much more rapidly than we are able to when we are too separated.

I am just giving this idea at the beginning of the meeting for your consideration. Towards the end of our sessions we can bring the thing up again and decide whether it is a feasible plan or not.

Now, going back to the subjects for discussion, I have asked Dr. Appleton to lead the discussion on the theory of wave propagation.

Dr. Appleton. — In accordance with Dr. Austin's suggestion I propose to review very briefly the present status of our knowledge of wave propagation with the primary idea of bringing up for discussion the main theoretical difficulties which still exist. As I think most of us are agreed that the Kennelly-Heaviside layer theory is a sound basis on which to build. I can go immediately to the first question of the physical process by which this layer deflects wireless waves and sends them back to the ground where we detect them. Are the waves reflected or refracted? We have, as is well known, the theories of Eccles and Larmor to assist us in explaining the latter case. But it was shown by the late Lord Rayleigh that reflection is marked when the degree of transition at a boundary is sharp within a wave length, so that we might reasonably expect long waves to be reflected and short waves refracted.

By that I mean that the degree of transition from non-conductivity to conductivity at the lower boundary of the layer may be sufficiently great within a fraction of a 20,000 meter wave length to produce true reflection. On the other hand in the case of a wave length of 16 meters we should have to fall back on the ionic refraction theory to explain the process of deviation at the layer, since we could hardly expect the ionization to vary very rapidly within a range of 16 meters. We thus see that there are two extreme cases of reflection and refraction and for some of our wave lengths probably both processes are present. There is no theory as yet for this intermediate case.

Within the last few years attention has been drawn to a factor which Eccles and Larmor omitted in their ionic refraction theories of short wave deviation. I refer to the action of the earth's magnetic field. This theory was worked out independently in America and England and appears to show that there will be a critical frequency for waves travelling through an ionized atmosphere. This frequency when calculated corresponds to a wave length of about 212 meters. For waves travelling along the earth's magnetic field there will be two circularly polarized components one of which will be strongly absorbed at the critical frequency, while the other does not exhibit any such critical absorption. Is the marked daylight absorption in the range of 200 meters to be ascribed to this magneto-ionic influence or to some other cause? Since the absorption on 200 meters is not so pronounced at night, and since Meissner failed to find any specially marked fading on this wave length, the applicability of the magneto-ionic theory in this case has been questioned. The problem cannot by any means be regarded as settled and I hope we shall discuss it with profit.

There appears to be another point in this connection that may be of importance. The theory of the general case of propagation in any direction to the earth's magnetic field shows that there are always two elliptically polarized waves propagated and that only one possesses critical characteristics. This critical component is always more strongly absorbed than the non-critical component so we have to consider whether the signal produced at the ground is not almost wholly due to the non-critical component. Such an explanation would explain Meissner's results but still leave the difficulty of the pronounced daylight absorption at 200 meters unsolved. I may mention that in some experiments on the polarization of downcoming waves carried out on 400 meters in England the waves have been found to be righthanded circularly polarized. This would correspond to the non-critical ray of the magneto-ionic theory for the case of the northern hemisphere. To test this point still further we require more data on the polarization of both long and short waves.

Another unsettled point, which is linked with the question of reflection versus refraction, is the nature of the trajectory of the overhead waves. Even if we limit ourselves to the cases of short wave propagation, in which we may assume that deviation is brought about by ionic refraction, we are not able to proceed very far with our theories for lack of experimental data. Various methods of finding the characteristics of downcoming waves have been developed both here and in England and I hope that perhaps combinations of these methods will tell us how far the ray has to penetrate the layer before it is bent back. This subject very naturally leads us to the question as to whether different wave lengths travel along the same path. Mr. T. L. Eckersley of Marconi Company has given a theory of wave propagation according to which, for the same two stations, the paths of overhead waves are always the same no matter the wave length. On the other hand, I believe that different wave lengths travel to different heigths, the short wave going higher, and that the low absorption of ultra-short waves is due to the large height at which they are bent. But, again, the matter is still not settled.

In cases of the transmission of short waves round the earth another somewhat allied problem arises. Are we to regard the ray which causes the signal as having been reflected to and fro between the layer and the ground or as having made the greater part of its journey in the layer. Dr. A. H. Taylor's data on short wave transmission seems to prove very definitely that the first process certainly exists but are we to rule out the second possibility because of this? The data on the multiplicity of signals caused by a short wave dot as observed by Quäck may perhaps help us here.

Dr. Austin. — Discussion on this subject is now open. I will be glad to hear from anybody who is ready to speak.

Dr. E. O. Hulburt. — With regard to the very interesting remarks of Prof. Appleton about the explanation of the dip in the range curve at wave lengths around 200 meters, it seems to me that the complete explanation will be rather complex. The point of interest is of course as to what extent this dip may be regarded as a proof of the action of the earth's magnetic field upon the wireless waves. Assuming that the earth's magnetic field is operative in the upper regions of the atmosphere, the propagation equations of waves variously polarized passing through these regions may be worked out. From the dispersion and absorption of the various waves obtained in this way it is seen that the behavior of the waves in the vicinity of 200 meters will be very complex, but there is an indication that this region of waves will be transmitted with less intensity than the longer and the shorter ones. This suggests that the dip in the range curve owes its existence to the magnetic field of the earth. The argument is none too conclusive, however, at the present time, for it is not hard to see that conditions might be such that the dip would occur even if the effects of the earth's field were negligible. For we might suppose that the rise of the range curve towards the shorter wave lengths was due to the increasing strength of the sky wave and the rise towards the longer wave lengths was due to the increasing strength of the ground wave.

Dr. Van Der Pol. — It is very easy to calculate on the basis of the theory given by Zenneck that absorption of the ground wave for these very high frequencies is exceedingly high so that when this theory is accepted waves of 50 meters would be absorbed at a distance of some 40 or 50 miles or so. Therefore in the case of short waves from distance there is no doubt that it is only the sky wave with which we are concerned. The dielectric constant becomes smaller than unity as their electrons are imprisoned in the atmosphere. I did some work on this in 1918 in Cambridge. I imitated in a glass tube an atmosphere like that supposed to exist high up in the earth's atmosphere and produced a discharge in this tube so arranged as to get electrons in the tube and to measure these short waves. I made measurements of the dielectric constant inside of this tube and got results showing that the dielectric constant was much smaller than unity and also that the dielectric constant was negative. A month or so ago I completed these experiments and got dielectric constants of 2 and I think this is a laboratory confirmation of Eccles and Larmor. Experiments in France recently seem to confirm these experiments of mine in 1918.

Dr. Breit. — Has anyone made an estimate of the intensity of the signal received on short waves over very long distances and compared that with what you would expect with reflections? It looks as if the intensity which one gets is very high. One might almost suspect that there would be a very strong focussing effect. People say that a man working South Africa can be received here if he sends on a 2-watt or a 5-watt tube. That means something very extraordinary. I should like to know whether anyone has a solution for that.

Mr. Pickard. — I can give one measurement made in 1925 at Seabrook Beach, N. H. of a station in South Africa working at about 37 meters with an input of 300 watts. The maximum field intensity was between 200 and 300 microvolts per meter. The mean field intensity over an interval of several minutes was approximately 65 microvolts per meter.

Dr. Taylor. — The question raised by Dr. Breit is one which is very difficult to answer on account of the focussing effect. It is not necessary to go to the antipodes to get this effect. Working from Washington to Honolulu (5,000 miles) is more difficult than working from Washington to Samoa (7,000 miles) and at the appropriate hour of the day it is still easier to work from Washington to Sydney (approximately 10,000 miles). It is convincing to me at least that there is a certain amount of energy concentration at great distances. Of course the true focussing effect at the antipodes is for short waves extraordinarily strong. We carried out a number of experiments between Honolulu and Johannesburg and they are exactly at the antipodes, and the strength of their low power signals at Honolulu from about 4 hours in the morning when they come across one ocean and 4 hours in the afternoon from the other direction across the other ocean, the strength of these signals was phenomenal and in every case when we got this focussing effect we believe that the fading is greatly reduced. There is not anything known about these signals and we have a lot to learn.

Dr. Wilson. — The maximum signals that we got during night were according to inverse law. Other signals, of course, are of lower intensity.

Dr. Taylor. — On account of the rapid variations that we got in signal strength in the higher frequencies as well as slow variations we got two types of fading. It is no use attempting microvolts per meter measurements except with automatic recording apparatus and on a statistical basis.

Dr. Van Der Pol. -- Our experiments confirm what Dr. Taylor said. In Holland the amplitude of the waves received in Australia, Melbourne and Sydney, are stronger and greater than the amplitude received in the Dutch East Indies. In Melbourne and Sydney we can beforehand arrange rebroadcasting in Sydney. It has always been a success and we have done this three or four times. I do not think this could be done in India.

Dr. Austin. — How does fading seem to be at these very great distances? Is it less than fading on the short waves at 3,000 or 4,000 kilometers?

Dr. Taylor. — Fading is enormously less. The high speed fading, audio frequency fading, at short distances, is conspicuous by its absence. Prof. Appleton told me that the Dutch programs received in England are much torn up. They are very much better at greater distances. There can be an understandable program on 64 meters in South America more readily that there can in New York or Washington.

Another point I would like to speak of here and that is the question as to just how that range curve behaves at night. The difficulty in determining what 212 meters would do at night is that it never does quite the same. It is an extremely erratic frequency for night work. That is why we handed it to the amateurs. There are different days when three types of signals can be observed in Washington. You get one signal which would appear to be a very, very weak ground wave. It requires a very sensitive receiver to pick it up. You get another signal which is what we call a « solid » signal, not broken up. We get another signal which occurs only at certain seasons of the year and certain hours of the day. This signal when once heard by an expert operator will never be confused. You will identify a round-the-world signal at once because you can hear the reverberations and reflections in that signal. It has a very peculiar psychological effect. I have no doubt in my own mind that a round-the-world signal comes around by the hippety-hop method. I think that a better analysis, perhaps a mathematical analysis of the oscillographic records of such signals such as were taken by Dr. Wachner in Berlin, on round-the-world signals would perhaps give us some clue to the history of the path. We cannot say that they are necessarily earth reflections but it seems probable that they are.

Dr. Wilson. — Some years ago Mr. Heising did some experiments in which he used methods similar to Breit's. He sent out pulses repeated every 60th of a second, and the signals as they arrived at a distant station were picked up by receiving apparatus and recorded on an oscillograph. He found that for transmission from Deal Beach, N. J., to a place on Long Island, he got just one pulse arriving at certain times of the day. At other times two pulses would arrive and sometimes three. On certain occasions he got as many as four, and one of the peculiar things about it was that when he got the four pulses arriving they were approximately equally spaced in time, suggesting a condition of multiple reflection. This was at a distance of about 40 miles. One experiment was done at a fairly long distance (about 1,500 miles) and the one pulse at the sending station gave six at the receiving station. These six were not, however, spaced equally but indicated that there was no simple connection between the various paths.

Dr. Breit. — We have also occasionally observed those double reflections and sometimes triple, but in our case it did not seem to correspond to any such reflection as that. Nothing that you could interpret as a wave going up and down.

Dr. Wilson. — Ours were more definite for the nearby stations. The experiments were made just after nightfall.

Dr. Goldschmidt. — I would like to tell you something of the result of experiments that we have made daily and nightly at a distance of 6,000 kilometers for three years. We had very much fading. At daylight and darkness you can use a wave length of about 6,000 kilometers.

Dr. Van Der Pol. — You have to increase the field strength eight times in order to increase the voltage of your detector two times, and this, I think, is the reason why the differences of the received amplitudes with a critical reaction set are so much smaller than the amplitudes received with a superheterodyne, and I think that this is a very important point in connection with the interpretation of the intensity of the received signal.

Dr. Breit. — The information which has just been discussed about the nature of fading of any distances supports an idea of Dr. Taylor that the fading of the sky wave is an interference phenomena. At a short distance you have two waves which differ considerably in their path. As soon as you deal with another distance you have the geometrical path of the wave not differing as much. It would rather look as if the sky wave at the long distances does not deviate because the two curves are very much spread out and their path difference is very small.

Dr. Appleton. — That fits in with some experiments on 400 meter waves, carried out in England, which seem to show that we probably were getting one ground wave and probably two or three rays of that type.

Dr. Bown. — There is a possibility that the actual motion of the physical gas may be responsible for some variation in the path.

Captain Bureau. — There are reasons which have been worked out in France to suppose that we are dealing with effects not only of reflections and refractions in the upper atmosphere but also in the lower atmosphere. In regard to the upper atmosphere, evidences obtained from a single emitting station indicate that a simple theory of either reflection or gradual refraction would be inadequate to explain the phenomena; instead of taking the classical theory of Larmor we have variations whereby there are maxima of ionization succeeded by partial minima of ionization and successive layers formed from which reflections and refractions may proceed. The different effects produced by different paths are capable of accounting for many of the irregularities produced. In regard to the lower atmosphere, there have been sufficient experiments made, by taking simultaneous measurements at various receiving stations in France for a single transmitting station, to correlate the results obtained with meteorological phenomena. Taking the transmitting station at Ainse and listening all around France, there are certain days when it has been observed that the reception has been good in all directions whereas upon the very next day for the east of France I have been quite unable to receive that station whereas the western side was received very well. The line of definition from the part that I did not and did receive was approximately a straight line north and south. Weather conditions affect this. I conceive that the deviation properties of masses of air would be capable of accounting for deviations in the direction of the radio action, which would account for the phenomena. Considering the optical reflections that occur we may assume that we have a state of affairs where the ground wave is entirely eliminated and where the action is entirely due to the reflections coming over the upper atmosphere. A little examination will show that a very small irregularity in the reflecting surfaces means that after the first layer of darkness has been passed the second layer will be much narrower and irregular, and so I do not suppose that the upper layers of the atmosphere are uniform and the second skip distances should be more irregular; evidence of that sort ought to be available to help us to decide.

Prof. Vanni. — I desire to present the question as to whether the theories we have been discussing seem to be adequate to explain the remarkable phenomenon that has recently been observed, that when a sending station A is transmitting to a receiving station B along the line, say, west-east, then the signal that is received at the eastern point is also followed apparently by a signal that has gone around the world in the opposite direction but comes in from the east at the receiving station. Can such reception be explained either upon the basis of a reflected or refracted wave or by any other adequate explanation?

Dr. Taylor. — This question of round-the-world signals is one that needs further investigation but as far as the data at hand is concerned, round-the-world phenomena occur at favored seasons and favored hours. For instance the work done on Radio Corporation signals received in Berlin on 16 and 22 meters began in the late fall of the year and about 4 o'clock in the afternoon central European time. That discovery of round-the-world signals came as somewhat of a surprise. We had feared that somewhere in the transit around the world the layer would be too high to permit those signals to come down again, but a study of the globe showed a fact of very great interest, namely that when it was 4 o'clock in the afternoon in Berlin it would be 10 o'clock in the morning on Long Island where the signals started. If you follow the Great Circle path at that time of the year you find that by the time you have come back to our west coast and your time has gone to 7 o'clock in the morning you are, say, down in the equator. It has passed from the region in which we would not suspect the layer to be even high enough to have cut the wave up. We have been examining round-the-world signals here and we find that the favored hour would appear to be near sunrise and sunset and in fall or spring. Because such a signal observed here to have a chance to get around through a layer of substantially equal height and never too high would want to come over the poles. Therefore it must come over during the half season at the poles. You will find it at the equinoxes and at that time of day when you can have daylight all around.

Major Blair. — In the laboratory with very short waves in getting interference phenomena I have suspended wires in front of the transmitter and I find that by adjusting the distance apart of these wires I have been able to reflect exactly half of the energy and bring it back to have it interfere with the other half. The spacing of the wires had to be more or less in proportion to the wave length. At the time these experiments were being done we did not know as much about transmission as we do now but by spacing the wires appropriately you may divide the energy. It is quite possible then that if there are a certain number of electrons at this level the 75 meter wave will be reflected very well and the 35 meter wave will go on up to some other point; generally I would like to say that every wave length will find its own reflecting layer in that way. I would also like to emphasize the influence of the lower atmosphere in the transmission of electric waves. We are getting data now which indicate that the lower atmosphere has a tremendous effect. We do not know enough about it to say very much yet but I am sure that we will find food for thought along the line that Captain Bureau spoke of in connection with the lower atmosphere.

Dr. Taylor. — I am also convinced that the lower atmosphere does play a role. We are finding very direct connections between weather conditions and reception in these high frequency points in the day-time. There is no doubt an influence there.

Major Blair. — In connection with some work on high frequency work on airplanes we have run into one or two peculiar things in flying over cities where we have reason to suppose that the dielectric properties of the air are quite different.

Dr. Austin. — We have formerly always supposed that the atmospheric effects on the signals took place at a height far beyond the limits of the flight of airplanes, but observations on the effect of cold waves on reception have indicated that there probably is some mechanism connecting conditions near the earth and conditions higher up which would explain the effect of one on the other.

Mr. Pickard. — I have recently attempted to correlate night reception in Massachusetts from a station in Chicago with temperatures along the entire path and also at the two ends. When 47 consecutive nights were compared with the mean temperature along the entire path a very low correlation was found. But taking the temperature in a semi-circle approximately 250 kilometers in radius around the receiver, and in a similar semi-circle around the transmitter it was found that the correlation between reception and temperature was much higher for the zone around the receiver than it was for the zone around the transmitter. This investigation is still in progress, and I hope that with the examination of a sufficient number of cases it may be possible to establish these relations more definitely.

It has been suggested this afternoon that there is a connection between certain fading phenomena and high-level air currents. If this is so, fading over the east-west path would be different from that over a north-south path, as it is well known that these high level currents move in a general west-east direction. But the velocity of these high level winds is not as great as has been generally assumed; in fact it may be even lower than at the surface. Although the data are scanty, being derived from the observed motions of sounding balloons at between 20 and 30 kilometers, and the drifting of meteor trains at heights of 65 to 110 kilometers, it appears that the maximum wind velocities in temperate latitudes are reached at a height of approximately 10 kilometers, where the mean velocity is about 17 meters per second. At 16 kilometers the mean velocity has been reduced to about 7 meters per second, while the extreme heights reached by sounding balloons, and the still higher meteor trains show velocities under rather than over those at the 16 kilometer level.

Dr. Austin. — Dr. Appleton has suggested that a special meeting be held tomorrow for informal further discussion of this subject. Such a meeting is called for 9 : 30 A. M. tomorrow.

Meeting adjourned at 4 : 45 P. M.

Meeting of October 17, 1927

Dr. AUSTIN, Chairman.

Dr. Austin. — So far we have been discussing for the most part short wave phenomena and the height of the Heaviside layer. I think it would be a good plan for us today to consider the questions of long wave field intensity measurements. I touched upon them briefly in my report. Perhaps the most important thing we ought to consider in connection with long wave measurements is the best way of getting accuracy. It has certainly been rather deplorable in the past that so many of the different experimenters have arrived at results which apparently were so discordant. I think I could say without danger of contradiction that some of the methods apparently have given average values which are at least five or six times as great as some of the other methods. It appears that these quantitative measurements of field intensity are among the most difficult which we have anything to do with in physics. In general we have to compare a very weak electromotive force which is impressed on the antenna with some artificial electromotive force which we can impress on it in our laboratories. We have no instruments for measuring this very small electromotive force directly, therefore we must measure a stronger electromotive force and then by some process, current transformer, resistance net, or mutual inductance, produce a certain amount of attenuation, the value of which we can assume with some certainty that we know. I do not know that we in all cases understand the problem fully. I do not feel that I do. We have a condition in which we have comparatively high electromotive forces in our source. Now we have to screen this source in such a way that there shall be no false couplings from the oscillator itself, to our antenna. We have got to be sure that we are sending nothing to our antenna except that which goes through our attenuation system. Now it would seem that that ought to be fairly easy to do by means of shielding and by properly grounding our shields, but it is my experience that this is a very difficult thing to do and so far as I can see now the best chance which we have for accuracy lies in trying a number of different methods and taking the methods used by different people or in different countries and in bringing those together and actually testing them and seeing whether we can get the same results. I do not feel from my own experience that we are able by any tests, which are at all simple, to be sure that any given piece of apparatus is giving the results which it is supposed to give. I would like to hear what some of the rest of you have to say about this.

Mr. Pickard. — I have had experience with several methods of comparing signal and locally generated current during the past few years and for the last year I have found, at least in the broadcasting band, that the best way is not to attempt attenuation of the locally generated current between its source and the resistance in the antenna but rather to effect a frequency conversion from an oscillator of different frequency than that of the signal, and so manufacture only the amount of signal frequency current required. The circuit which I have been using is that devised by Dr. Van Roberts, in which a half frequency oscillator is variably coupled with a full wave rectifier acting as a frequency doubler. The value of the rectified double frequency current may be directly read by a d. c. microammeter because if the rectifier tubes are properly biased the root mean square value of the pulsating current which is read on the d. c. microammeter is also the speak value of the alternating current. I have checked this method repeatedly and it agrees with a carefully designed and shielded oscillator and attenuation network to better than ± 10 %. It has the merit that only a rudimentary shielding of the half frequency oscillator is required.

Dr. Appleton. — The subject of accurate wave measurement is hardly within my province but one method I have used in connection with the broadcasting band may be of interest, and that is to attempt to imitate a small transmitter. Instead of using a local oscillator and having the difficulty that Dr. Austin has emphasized we use a small perambulator with a little wireless set on it of which we know the current and by means of induction field used we can calculate the field intensity and in that way actually leave our receiving apparatus as it is and check the signal. It has many advantages

Dr. Van Der Pol. — In connection with the perambulator method. We have found it always very difficult when you have a very loose magnetic coupling between two circuits to be sure whether the electromotive force in one of the circuits is due to magnetic or capacitive coupling and it would be interesting to hear from the experts here present whether they know of any method with which one is able to say whether the loose electromotive force in the second circuit is due to a magnetic coupling or is it for electrostatic coupling for when the coupling is very close one is practically sure that 99 % of the coupling is magnetic but especially when this coupling is very loose I am not quite sure whether one is working with magnetic coupling only. And if this happens to be the case then of course you can not calculate the impressed electromotive force simply by calculating in the mutual inductance between the two sets and in these circumstances there is some probability of an error occurring.

Dr. Appleton. — It occurred once in some experiments I was engaged in. I put one coil inside a glass with a layer of silver so that the magnetic force were able to penetrate and the silver acted as an electrostatic shield but I do not think we actually used that method in the particular problem we worked on. The method used was to short each wire within the loop itself with enclosed loops of wire all connected to earth.

Dr. Dye. — Current transformers are used there and they are toroidal windings which lend themselves to electrostatic shorting. Also the magnetic coupling is so perfect that in every minute coupling might take place. It is of course very difficult to determine whether a particular method is giving results or not but of interfering with other methods and in that respect the method used by Dr. Appleton can be considered a method in which coupling of some kind is used. When this system is being used to receive signals it is a full wave which is passing which produces effects... We can either work well within the critical point of 1/6 of a wave length which divides the induction field from the radiation field, or we can get well beyond that point to a full wave length and assume that you are getting a true radiation field.

Dr. Dellinger. — It is very easy to find fault with any method that is proposed or used for measuring field intensity. It is a very difficult type of measurement. If the transmitter which is used to furnish the voltage is one wave length or more distant it is probable that the capacity coupling is negligible. But when you are using an induction field to supply the voltage you do have to adopt very great precautions as to capacitively introduced emf's. I can cite three different lines of evidence to prove that. In working with frequency meters you can very greatly change the deflection of your deflection instrument by connecting and disconnecting a ground wire. Then as a second line of evidence in making practical measurements on sensitivity and selectivity of receiving sets when you are trying to induce a known voltage you must use careful shielding. Then in the third place when we take this problem itself of introducing a known voltage in the antenna circuit of a field intensity apparatus many here present know that you have to go to extreme precautions in introducing heavy shielding to prevent the voltages going into the apparatus in the wrong place. For those reasons it is frequently desirable to use the other method, i. e., working with a voltage which is supplied from a distance by radiation, but that method is not free from difficulties. In order to be sure that you are calculating voltage your transmitting condenser antenna would have to be isolated in space and so would the receiving apparatus. Just what function the earth performs it is almost impossible to calculate. In experiments it is necessary to take rather elaborate steps to find this out. I cannot agree that it is a different kind of voltage that we get in the two cases; a voltage is a voltage and I do not think there is more than one kind. In the one case you may be getting a different voltage but I do not believe that we need think of an actual physical difference in the voltages produced in the two kinds of experiments.

Dr. Van Der Pol. — Once we had a small loop at a distance of half a meter or so and used this receiving set and transmitting set 8 or 9 years ago. We measured induced electromotive force in the circuit with 2 loops of wire 1/2 centimeter diameter. Reversed connections of transmitting loop and obtained very different results. This is a clear indication that there is electrostatic coupling.

Prof. Mesny. — Pour les mesures de rayonnement j'utilise un couplage électromagnétique entre l'oscillateur local et le cadre de réception. Ce couplage est établi de telle sorte que l'inductance mutuelle varie proportionnellement au simus de l'angle dont on fait tourner la bobine mobile; cela facilite les opérations d'étalonnage et permet en outre de constater que les couplages électriques sont négligeables, il suffit pour cela de vérifier que la loi sinusoidale est bien suivie.

En outre, je me suis débarrassé de toutes les difficultés provenant des fuites à travers la cage qui renferme l'oscillateur local en fermant cette cage avec un couvercle en cuivre dont le bord amalgamé trempait dans un bain de mercure. Ce mercure est contenu dans une gouttière en cuivre amalgamé et soudé à la boîte près de son bord supérieur; l'amalgamé est indispensable.

Dr. Austin. — Of course the problem is considerably simpler in the case of the longer waves. To tell the truth, I have never been quite ready to tackle the problem of the shorter waves myself. Mr. Pickard has done a lot of successful work along that line but we have had so much trouble in satisfying ourselves that we really are getting the correct results even on the very long waves that we have not reached the point where we are particularly anxious to try anything which is more difficult. I really believe that if you can know the effective height of a transmitting antenna with some degree of accuracy which is not far enough away so that the reflection is likely to come in as a disturbing element, that the electromotive force which you get from the waves from such a transmitting station at a few wave lengths distant must be the most accurate method of calibrating your receiver. It is not a convenient method, but where such a station is available I believe that it is the most accurate source of calibration.

Mr. Pickard. — I would like to ask both Dr. Austin and Prof. Appleton what their experience has been as to the effect of obstacles between their nearby transmitter and receiver. I am urged to that question because in a recent attempt to get an accurate idea of the effective height of my receiving antenna I moved a loop transmitter around this antenna through a complete circle taking 15 or 16 observations during the circuit. I live in a residential district with buildings well separated but with quite a number of trees, and the results of this measurement indicated a nearly two to one variation although the radius was kept quite rigidly constant. The wave length used corresponded to approximately 1,000 kc (300 meters).

Dr. Austin. — That is a short enough wave so that I think that it is quite possible that the ordinary obstacles might play an important part. When working in the vicinity of 10,000 meters, however, my impression is that the ordinary type of obstacle does not make very much difference.

Dr. Kennelly. — Kindly tell us whether in that case you had 600 meters at some distance and whether in that distance there were houses or trees.

Mr. Pickard. — My mobile transmitter was approximately 2 wave lengths away from the receiving antenna and trees, houses and wires intervened.

Dr. Appleton. — I think there is some information available that would interest Mr. Pickard from some of the experiments carried out under the Radio Research Board. Barfield has taken very accurate measurements on 300 meters and found very interesting results. He attempted to plot contours going all around the transmitter. There appeared to be two marked sources of absorption. One is the density of trees in the path of the waves. He found the actual effect of any given tree in a very interesting way. He used a coil antenna quite close to a tree and assumed that the tree would act as a relay and in that way produced horizontal magnetic force. By finding the deviation quite near that tree he was able to estimate what was the local magnetic force introduced by the tree and he finds that the absorption is quite big enough to produce an effect. He also finds that he gets the strongest absorption when the waves pass over the regions most densely wooded. There is another thing and that is in a large town we have all the little aerials taking their own little bit of energy away from the wave as it travels out and so we would expect absorption. Barfield made arrangements with the British broadcasting corporation to change the wave length of the London transmitter and so found that his signal intensity went through a minimum. There are two quite different absorption factors which must be taken into account, trees and the number of wireless aerials around the transmitter.

Dr. Dellinger. — The effect of trees is very great. Perhaps an even more striking demonstration of an effect of an individual tree was shown in experiments at the Bureau of Standards when some attempts were made to produce a resistance-free antenna. This succeeded but only after a very great deal of experimental work had gone through. You can make a very good antenna but if a tree is in its field the resistance can not be kept low.

Dr. Austin. — This question of ground absorption is one into which we have not gone very far as yet. We do not even know whether at wave lengths of 10 to 12,000 meters or more, the transmission over land is materially different from the transmission over sea. The reason that we do not know, is that we are never very sure in regard to the effective height of the transmitting antennas with which we are dealing. If we once knew that value accurately in regard to the transmitting stations and then knew the current which was being used in the antenna, then we could settle all these other questions very easily, but while, of course, we have methods which are apparently fairly good for determining effective height, at the same time there is in the case of most stations a certain degree of uncertainty as to its actual value. I think that this is another thing, which we probably cannot go very far with at this meeting, but that we ought to have it in mind and try to improve our knowledge of the subject.

Dr. Van Der Pol. — Did you ever find effective height to vary as a function of the wave length?

Dr. Austin. — It does vary somewhat. I have worked over such a comparatively limited range of wave lengths that I have not very much experience in regard to that. Our work is practically all confined to stations between 8 and 20,000 meters and over that range apparently we have not discovered any definite sign of change. All these antennas in question are probably used some little distance from their fundamental wave lengths and it is only as you approach the fundamental that you get anything like a rapid change. Does anyone present have any information on that question?

Dr. Appleton. — Not on frequency but on the distance at which the effective height is measured. There is a remarkable effect there. After 20 wave lengths the radiation from the top of the antenna goes down very low on the ground at an angle at which there is no longer any further reflection from the ground itself. From 2 to 20 wave lengths, the product of signal strength and distance increases with the distance.

Dr. Austin. — It was evident from our first experiments made over sea water that we had a regular falling off nearly in proportion to the distance up to about 200 miles. We did not find any of these anomalies now found overland. Of course we did not imagine at the time that the earth was going to bother us as much as it has shown itself capable of doing.

I would like to ask in those experiments varying in distance from 2 to 20 wave lengths if there was any particular distance at which you found the height which you expected from calculation.

Dr. Appleton. — There is no evidence there. We really do not know the height. Measurements were made on a broadcasting station which was perched on top of a building.

Dr. Dellinger. — This discussion that was partly begun on effective height might be a good subject to discuss among others at the next meeting of the Commission on measurements. Of course you can place the dividing line where you please in discussing this subject but I doubt whether that particular subject would be very profitably discussed further here where we are interested in phenomena rather than methods of measurement. There is quite a difference of opinion as to this question of effective height, and I think it would be a question for us to exchange our ideas about in the other committee.

Dr. Austin. — I think that is a very good suggestion. There are so many things which can be brought up in this committee. So many differences of experience of the different workers and in the different countries that one hardly knows what to bring up in the rather limited time that we have here.

We think these experiments by Mr. Hollingworth and the Radio Research Board on the variations of signals with distance at very long wave lengths very interesting. I refer to those experiments in which the signals from Ste. Assise were found so much stronger in Aberdeen than in Manchester. If I remember the intensity was much below the assumed value in Manchester and rose again and so that not only was it higher than the calculated value but actually higher in Aberdeen than in Manchester. That is something we have not found at all in the day-time in this country. At night we can duplicate that sort of thing very readily. We have times during which we get a reflected wave which is in practically opposite phase to the ground wave. We then get very low values, almost zero signals, from the Radio Corporation station on Long Island, which many of you saw the other day. I presume if we could get still farther away we should find a high value, but I think the remarkable thing about these experiments in England is that this phenomena appears to come in the day-time. Have any of the English delegates any further information which can be given in connection with that?

Dr. Appleton. — Mr. Hollingworth's results do demonstrate that you really have this true reflection from the Heaviside layer. I use the word reflection for the reason because it does appear that these long waves are truly reflected in this way. When the layer is found to rise at night and the height is found the reflected ray is found to have abnormal polarization. It is found that the plane of polarization is actually rotating. Experiments were done many years ago by Kerr in which he showed that if light was reflected at one of the poles of the magnet you actually got polarization. That seems to have been actually observed by Mr. Hollingworth but you will note that the effect is quite different from the effect thas is observed on the shorter waves.

Dr. Austin. — Would you think that the difference in latitude of England and this region might possibly explain the effect that you get from reflection in the day-time. It has occurred to me that this might be a matter of latitude. You have it distinctly in England and not here. We have observed at various distances so that I think we would have known if we had any such effect.

Dr. Appleton. — Mr. Hollingworth would have to proceed pretty rapidly from one point to another so that the layer would not alter.

Dr. Dellinger. —- These measurements were made by actually proceeding from one place to another. Was a whole set of observations made in a day?

Dr. Appleton. — Yes.

Dr. Austin. — There is another point in which we have found results slightly different from yours in England and that is in the effect of magnetic storms at various distances. I think you find this effect only at less than 300 km in England. At our stations at from 435 km to 660 km we get about the same results that we do on stations which are at 250 or 260 km.

Dr. Appleton. — It is the short distances that show up the magnetic storms. The cases are opposite with reflection and refraction.

Dr. Austin. — We have not been able to find any increase in atmospherics with increasing solar activity. We have looked for it very carefully in our records. We have dealt more with sunspots than with magnetic storms, and of course on long waves only. That is another interesting thing that we shall have to check up our differences on. It begins to appear as though there must be considerable difference in the behavior of all these things when observed in different places and it may be a matter of latitude. As we get nearer to the region where the earth's field is more vertical we may have different effects from those that we get as we go farther south. We are really much farther south here in Washington than you are in England.

Dr. Dye. — The atmospherics you receive here are largely of different character than those we get. No reason to suppose they should be treated the same way.

Dr. Austin. — We are opposite Spain here.

Dr. Appleton. — The atmospherics received here come a short distance and you would not expect to be so much affected by an ionized wave. In England they come from a long distance and therefore propagation is affected very largely by the ionized layer and that may account for getting magnetic storm atmospherics and not getting them here.

Dr. Austin. — We need stations to observe wave phenomena at least as numerous as magnetic observatories.

Prof. Mesny. — Je tiens à signaler une fois de plus un phénomène qu'il ne me paraît pas possible d'expliquer avec les ondes réfléchies par la haute atmosphère.

On sait que sur terre les relèvements radiogoniométriques deviennent tout à fait mauvais à la tombée de la nuit. Pour préciser, une onde amortie de 4 à 500 kc est en général relevée de jour avec beaucoup de précision et donne lieu à une extinction rigoureuse, même si les signaux sont très forts. De nuit, si l'onde a parcouru plus de 25 à 30 kilomètres au-dessus de la terre il est impossible d'obtenir une extinction et la direction trouvée est fausse.

Au contraire si le trajet de l'onde se trouve entièrement au-dessus de la mer, les observations de nuit sont identiques à celles de jour et l'extinction reste absolue.

Prof. Appleton. — We can distinguish the direction of the ground wave from the direction of the downcoming wave. With over-sea transmission there is a strong ground wave. Apparent direction is determined by the ratio of ground wave to reflected wave. Since the reflected wave is polarized the apparent direction is rotated from true direction.

Meeting of October 26, 1927

Dr. AUSTIN, Chairman.

Dr. Austin. — I would like to bring up for discussion now the following report, which has been circulated before the meeting :

Suggestions for The International Study
of the Correlation between Radio Transmission
Phenomena and Solar Activity.

It seems to be now certain that there is a well marked connection between the strength of radio signals and solar activity. In the study of this correlation the following solar phenomena may be considered:

1. The general radiation (solar constant) and ultra-violet light in particular.
2. Sunspots and their 11-year cycle, faculae, and prominences.
3. Local solar magnetic fields.
4. Sudden solar outbursts.
5. Alternation of magnetic polarity of sunspots in successive 11-year cycles.
6. Absorbing material thrown out by the sun.

The are also the following terrestrial phenomena which are believed to be controlled largely by solar activity and which may perhaps be used to a certain extent as a measure of its intensity:

1. Magnetic variations (character of days and diurnal range).
2. Earth currents.
3. Atmospheric electricity (potential gradient and ionization of the atmosphere).
4. Aurora.
5. Amount of ozone in the atmosphere.

The study of most of these subjects has been much more highly developed than that of radio signal intensity. The suitable observational radio material is very limited and in any continuous and reliable form it certainly covers no more than the last five years and is confined almost entirely to wave lengths above 5,000 m., therefore it seems highly important to undertake the following measurements on signal intensity taken preferably at least three times daily:

1. At various wave lengths.
2. In different portions of the earth (observing points in the far north or south may prove of special importance).
3. At various distances.
4. In different directions.
5. For all daylight path.
6. For partial daylight path.
7. For all darkness path.
8. At sunrise and sunset.

In comparing signal strength with the other natural phenomena it would seem advisable to make use of:

1. Day by day comparisons.
2. Short period averages (3-10 days).
3. Monthly averages.
4. Yearly averages so as to cover eventually the 11-year sunspot cycle.

In all of these, straight averages, moving averages and periodic averages (period of solar rotation and others) would all seem to be probably useful.

While absolute measurements of signal intensity in microvolts per meter are desirable, relative measurements where the sensitivity of the apparatus can be kept constant will also be of value.

It will be important to have these observations published or at least distributed promptly through a central office for the information of other workers in this field.

The importance of this work should be called to the attention of governments and research organizations.

L. W. AUSTIN,
President of the Commission.

I would like to know if there is a wish to have this discussed further at this meeting. Do you want me to go into the particulars of these suggestions? The suggestions in themselves are quite general. We are entering upon a field about which very little is known and in this report I have simply enumerated the different observations which might be made and the different phenomena which might be studied and their possible bearing on the radio transmission. I do not feel as though there was very much to be said in regard to these suggestions except to lay them before the committee for examination. I think perhaps the most important point is at the close. I feel that the importance of this work should be called to the attention of the various governments and research organizations. I do not think that beyond what the report contains there is anything to be recommended. We must begin at the beginning. We have to gather together all the information we are able to on this matter of correlation and we ought to interest just as many scientific bodies as possible in this study.

General Ferrié. — We can bring this matter up in the General Assembly. It is very important.

Dr. Austin. — This proposition on the effect of solar activity was the only one which I thought would necessarily come before Commission II for action.

Captain Bureau. — Je crois utile d'attirer l'attention sur des *influences géographiques* dans la propagation des ondes courtes. Ces influences ont tout d'abord été constatées sur le *Jacques Cartier* à l'occasion des essais de transmissions et de réceptions de messages météorologiques faits à bord de ce navire. Pendant tous les voyages accomplis par ce navire depuis plus de trois ans, et effectués toujours sur le même itinéraire et aux diverses saisons de l'année, on a constaté que dans l'Est du golfe du Mexique, ainsi qu'à l'Est de la Floride et du cap Hatteras la liaison sur ondes courtes avec la France, soit dans un sens, soit dans l'autre, était beaucoup plus irrégulière que dans les autres régions de l'océan. Par contre les autres régions du golfe du Mexique, la mer des Antilles et surtout la région de Panama semblent très favorables à une liaison régulière avec la France à des heures qui dé-

pendent de la longueur d'onde. En particulier les émissions faites vers 7 heures Greenwich, et en hiver dans la région de Panama sur des longueurs d'onde de 55 à 120 mètres sont reçues en France avec une grande puissance (expériences faites à bord du *Jacques Cartier* et de la *Jeanne d'Arc*).

Des expériences récentes faites à bord d'un troisième navire français *La Ville d'Ys* semblent montrer que la zone de liaison irrégulière et difficile s'étend vers le Nord-Est dans les régions de Terre-Neuve, du Saint-Laurent et sur l'océan à l'Est de ces régions.

Il semble ainsi que les influences géographiques puissent se manifester également en modifiant la forme de la courbe de variation diurne de l'intensité de réception. Je signale ce fait simplement à titre d'indication, n'ayant pas encore pu faire à ce sujet un nombre suffisant d'expériences : les expériences répétées aux diverses saisons et pendant trois années par le *Jacques Cartier* ont montré que l'onde de 31 mètres jouait au delà d'une distance de 1,800 milles, le rôle d'une onde de nuit (route de France au golfe du Mexique en passant par les Açores). Pour une distance exactement identique, mais sur une route plus septentrionale (Le Havre à New-York) j'ai constaté au cours de ma récente traversée que l'onde de 31 mètres émise par Paris présentait un caractère inverse et s'affaiblissait très nettement quand la nuit venait recouvrir le parcours de l'onde.

Dans le même ordre d'idées, le capitaine STAUT a constaté d'une manière courante que, en Afrique occidentale française il n'y avait pas de zone de silence sur les ondes d'environ 30 mètres.

Il semble donc bien que la latitude, et peut-être la situation géographique, peuvent modifier très profondément les courbes de variations diurnes relatives à la propagation des ondes.

Dr. Austin. — It seems to me where we deal with these differences in different parts of the earth and under different circumstances, we must go through the same experiments a number of times before we can draw any conclusions, because radio is variable above everything else. If we find that on repeating these experiments four or five times that they always show the same thing then we have a problem that we can begin to work with.

How about experiments in the vicinity of Java on the short waves?

General Ferrié. — There will soon be many observations in North Africa and we can tell then. In South Algeria and in Senegal there is very good propagation of short waves. Very short waves are found to be very good in tropical regions.

Dr. Austin. — That certainly is a very important fact, and it would seem as though it should be of use in solving the problem of the mode of transmission. If we have two parts of the world in which we get opposite results it would show that the simple theory which has been made is not sufficient. Perhaps a difference in the height of the Kennelly-Heaviside layer would explain this, but it would have to be very different in its height in order to explain it. The theoretical people like Dr. Van Der Pol and Dr. Appleton should take this matter up and determine what would be the conditions required to explain it.

Bruxelles. — Imp. Hayez, rue de Louvain, 112.

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RECUEIL DES TRAVAUX DE l'Assemblée Générale tenue à Washington en Octobre 1927	PAPERS OF THE General Assembly held in Washington in October 1927

FASCICULE 5

Travaux de la Commission des Perturbations Atmosphériques (TEXTES ORIGINAUX)	Papers of the Commission of Atmospherics (ORIGINAL TEXTS)

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Recueil des Travaux de l'Assemblée Générale tenue à Washington en Octobre 1927

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TRAVAUX DE LA COMMISSION DES PERTURBATIONS ATMOSPHÉRIQUES

(Textes originaux)

La Commission des Perturbations atmosphériques de l'U. R. S. I. a été instituée par décision de l'Assemblée générale de l'U. R. S. I. tenue à Bruxelles en juillet 1922.

Les travaux effectués en cette matière par chaque Comité national ont été résumés et discutés, lors de l'Assemblée générale de l'U. R. S. I. tenue à Washington en octobre 1927. On trouvera dans ce fascicule 5 l'ensemble de ces discussions et rapports.

La Commission des Perturbations atmosphériques est composée des savants suivants :

PAPERS OF THE COMMISSION OF ATMOSPHERICS

(Original Texts)

The Commission of Atmospherics of the U. R. S. I. has been instituted by the decision of the General Assembly of the U. R. S. I. held in Brussels in July 1922.

The works made in such matter by every National Committee have been summarized and discussed, during the General Assembly of the U. R. S. I. held in Washington in October 1927. This part 5 includes the complete discussions and reports.

The Commission of Atmospherics is composed of the following savants :

Belgique :	Colonel A. WIBIER, Directeur général de la T. S. F. au Congo Belge.
	M. JAUMOTTE, Directeur de l'Institut royal Météorologique.
France :	Général FERRIÉ, Chef de la Télégraphie Militaire.
	M. Marius LATOUR, Ingénieur.
	M. le Prof. MESNY, du Laboratoire de la Radiotélégraphie Militaire.
	M. ROTHÉ, Professeur à la Faculté des Sciences de Strasbourg.
Great Britain :	Prof. W. E. ECCLES, F. R. S.
Italie :	Commandant J. PESSION, de la Section des Radiocommunications au Ministère de la Marine.
	Colonel SACCO, Directeur de l'Usine Radiotélégraphique de l'Armée.
	Dom Bernardo PAOLINI, Directeur de l'Observatoire Météorologique de Monte-Cassino.
Norvège :	M. BJERKNES, Professeur à l'Institut Géophysique.
	M. le Capitaine SKOLEM, de l'Admininstration des Télégraphes.
	M. O. DEVIK, Sous-Directeur de l'Institut Géophysique.
Portugal :	Commandant Nunes RIBEIRO, Chef des Services Radiotélégraphiques de la Marine Militaire.
United States :	Dr. L. W. Austin, of the U. S. Naval Radio Research Laboratory.
	Major General George O. SQUIER, of the War Department.

Les séances de la Commission, à Washington, étaient présidées par M. le Prof^{r} Mesny.

The meetings of the Commission, in Washington, were presided at by Prof. Mesny.

Meeting of October 14, 1927

Prof. MESNY, Chairman.

Prof. Mesny. — We have to examine a whole group of observations and theories of the greatest interest, and I think that in order to best utilize the short time at our disposal, we should begin by considering the nature of the different questions we are to study and by methodically classifying them.

The phenomena relating to atmospherics may be looked upon from a double point of view : the radio aspect, which we meet only when we are interested in technical results; and the purely physical aspect which appears when we look for their origin and the relation they bear to the general physical nature.

In both cases very special methods of observation and analysis are used, and a discussion of these methods should be carried on carefully.

RADIO ASPECT. — In this part of our work we shall have to examine the different types of disturbances (called « clicks » and « grinders »), their intensity, the frequencies on which they are heard most, the frequency with which they are produced under different conditions, the direction from which they apparently come most often, and finally, the effect they have on the inaudibility of the signals according to the speed of transmission.

I wish to call attention to the uninterrupted work on this subject carried on by Dr. Austin, who has been a pioneer in this field as in many others, and I also wish to recall the important work of Messrs. Watson Watt and Herd, of Mr. Friis, and that of Mr. Bureau, and that of Messrs. Eckersley, Round, Tremellen and Lunnon, who have all accumulated a large number of precise observations. Mr. De Bellescize has made a very thorough discussion supported by numerous observations on the influence of the speed of transmission on the inaudibility of signals in the presence of atmospherics. Messrs. Judson, Yokoyama and Nakai have also brought to us in this session of our Union the results of quite recent investigations.

PHYSICAL ASPECT. — Although all the knowledge that we have on atmospherics is intimately related in its consequences, it is rather the physicist who is interested in studying the nature of disturbances, the energy which they put in play, the range of the waves they give rise to, their seasonal and daily variations, and finally, their origin and source.

Very thorough studies of these questions are under way, and very different hypothesis have been suggested. The scientists who have devoted themselves to them have spared neither efforts nor ingenuity in seeking the support of innumerable observations. All of those people I mentioned a moment ago have brought a contribution to this work. I should also add the names of Messrs. Appleton and Baumler.

Two apparently opposite theories are found; one is that the origin of atmospherics is very far away and located over the continental regions adjoining the equator; the other is that they originate very near the places where they are felt.

We are fortunate enough to have with us, Mrs. Appleton and Bureau, who are defenders of these opposite theories. This will save my going into this any farther now, I hope it will be discussed by them fully. Let us hope that some general ideas will develop from our discussions which will allow us to bring about the crucial experiments which would decide the difference.

METHOD OF INVESTIGATION. — For a long time the observations have all been exclusively aural. The method is indeed subjective; it has, however, given some results, and it seems, can furnish more results. Our discussions will enlighten us on this point.

For several years recording apparatus in various forms has been used. It undoubtedly gives surer results but its use is more delicate, and naturally this apparatus has been rather expensive. We shall try to decide what are the most interesting ones to be recommended.

Whatever be the conclusions we shall reach on this matter we cannot fail to remark the ingenuity with which oscillographic methods have been used by Mr. Appleton and Mr. Watson Watt. The radio direction finder with instantaneous recording designed by Mr. Watson Watt seems likely to furnish important results.

In order to go back from the observations to the original phenomena it is not enough to accumulate experiences. A strict analysis is indispensable. This analysis may sometimes be done in a qualitative way, but more often we shall have to go to the numerical values and even as far as the mathematical analysis. This is for instance the case if we wish to draw useful consequences of the results furnished by the receiving sets. We must find which are the electrical disturbances susceptible to bring about the troubles that have been observed and further examine whether such disturbances appear as likely. Mr. Moulin, on the one hand, Messrs. Burch and Bloemsma, on the other hand, have treated part of this question.

We have before us, as you see, a vast program. I suggest we start with the last part, so that we shall know exactly what are the sense and value of the observations when we study the phenomena itself. Moreover, we shall pave the way for the organization of international observations, of organization of which is also a part of our program of work.

Mr. Friis. — I think it best to discuss here the method of recording atmospherics, to see whether we can agree about some common method of recording in the different countries, and to compare results obtained in France, England, United States and Japan, etc.

Dr. Appleton. — The British Committee have some proposals to make but I really ought to extend them in view of Mr. Friis' suggestion. Probably the kind of thing he would suggest each country having would be some method of determining the atmospheric intensity on different wave lengths because from a practical point of view the radio engineer is not interested in the form of discharge. The practical engineer is interested in the energy spectrum. I imagine that you would be in favor of having some definitive method of taking a series of wireless receiving sets at different wave lengths and having the amount of energy for that particular wave length. Proposals of the British Committee which I will mention later really are dealing with standard methods of getting wave form and intensity and in particular the direction of arrival of the individual atmospherics. That method is now working very beautifully and Mr. Watson Watt has made the thing work quite perfectly. Mr. Watson Watt can by using two stations, triangulate and locate origin of atmospherics. They are even afterwards able to compare them with the weather reports and the results are most astounding. I have brought with me one of Mr. Watson Watt's charts which I would like to show to anyone who is interested. Cities which were found by these two stations indicating region of atmospherics and the points where thunder was recorded during that same day are also found on the map.

Prof. Mesny. — Does anyone desire to speak of general recording of atmospherics?

Dr. Austin. — We have a good deal of trouble in this country in regard to the matter of just what we record when we record atmospherics. If we take them with a slow moving instrument we get one thing. If we take something which records just the peaks (the clicks), such as Mr. Watson Watt has been recording, we seem to get something else and the directions do not by any means coincide. Whether there is any real difference in the nature of the different types of atmospherics I do not know, but I imagine that it is something like this. Those special types which you get from local lightning flashes are very apt to show separate high peaks, while those which come from the regions in the tropics where there is a continuous disturbance going on, whether due to groups of thunder storms or some different cause, come in rapid succession in such a way that we get integrating effects which give curves quite different from the thunder storm clicks of the region within 400 or 500 miles. I think that the conditions in England and in Washington, for instance, are quite different on account of the differences in latitude. I think probably in England you have a comparatively pure, unmixed type of atmospheric which makes it much easier to get conclusive results that agree with each other.

Dr. Appleton. — Might I ask you if it is the big clicks which show bearings all the way around?

Dr. Austin. — No, not always. Sometimes these peaks show very good direction and they apparently are connected with thunder storms. Then we get those other things which correspond more nearly to what have been sometimes called « grinders », which seem to come from a great distance and yet more or less from fixed points. For instance, during the winter we get these grinders from the southeast, that is, perhaps from the eastern part of South America or even from Africa, and then as the season goes on and the sun begins to move up north so that the land areas which are more nearly under a vertical sun lie more to the south and even to the southwest of us there is a shift in the general direction. Of course it is not a universal rule but it seems to be the travelling march of the sources of these grinders which we get generally on our integrating instruments. When we try to record the peaks in the summer we are very apt to pick up something which appears to be more or less local in its nature. That is the reason why we have never thus far settled down to any method of recording, because we have been bothered by having these two kinds of effects which we have not known quite what to do with. We have not felt that the simple counting method such as Mr. Watson Watt has used was satisfactory for this region. I suppose, as a matter of fact, that we really ought to have instruments which would take both and in that way study out their differences and finally make up our minds as to what their real sources are. I think that the grinders are of the greater practical interest. These, on the whole, during the summer, come from the southwest and seem to be the disturbances which cause the most trouble in the reception.

Mr. Alexanderson. — A discussion of methods of recording atmospherics makes me think that the natural development of radio communication may automatically devise a method for that. The tendency now is to develop photographic methods. Several forms of this type of apparatus have shown that very good photographs can be transmitted by radio. Here is a sample of a message transmitted at 200 words a minute. It is expected that radio transmission in the future will be done by taking a hand-written paper and putting it into the machine and it will come out as a fascimile. This will show atmospherics. We will have on these communication messages an accurate picture of the disturbance. Disturbances lasting 1/1000th of a second are recorded. The machines for transmission and reception must be synchronized. We might not only synchronize a single transmitting and receiving station but we might distribute a time service by which all the machines all over the world would run synchronously to 1/1000th of a second. A collection of the messages transmitted under such a system might give us the real solution to the method of recording atmospherics.

Prof. Mesny. — Cette méthode est très intéressante; mais en ce qui concerne l'influence des atmosphériques sous les tropiques, je crois devoir faire remarquer ici qu'il existe un procédé très simple, souvent employé et qui donne des renseignements précieux. Ce procédé consiste à enregistrer des signaux quand il y a des atmosphériques; on compte ensuite le nombre de lettres qui ont été brouillées et on calcule le pourcentage des signaux illisibles. La simplicité de ce procédé permet de l'appliquer très généralement en tous lieux et en toutes saisons pour obtenir des résultats entièrement comparables entre eux.

Captain Bureau. — Following up the remarks of Mr. Mesny, two methods have been tried in my laboratory for registering atmospherics. The first method uses a relay which is arranged to close its circuit as soon as the intensity of the atmospheric attains a certain magnitude, and auxiliary apparatus takes account of the number of responses of the relay in a given period of time. The second method uses rectified current through a condenser in such a way that the amplified current of the condenser shows the integrated value of the atmospherics during a certain time. These two methods, although they differ widely, appear to give concordant results and two sets of curves are obtained, one by the relay method and the other by the condenser method. I will now give details of these two methods, and of another method of measurement of some interest in connection with atmospherics.

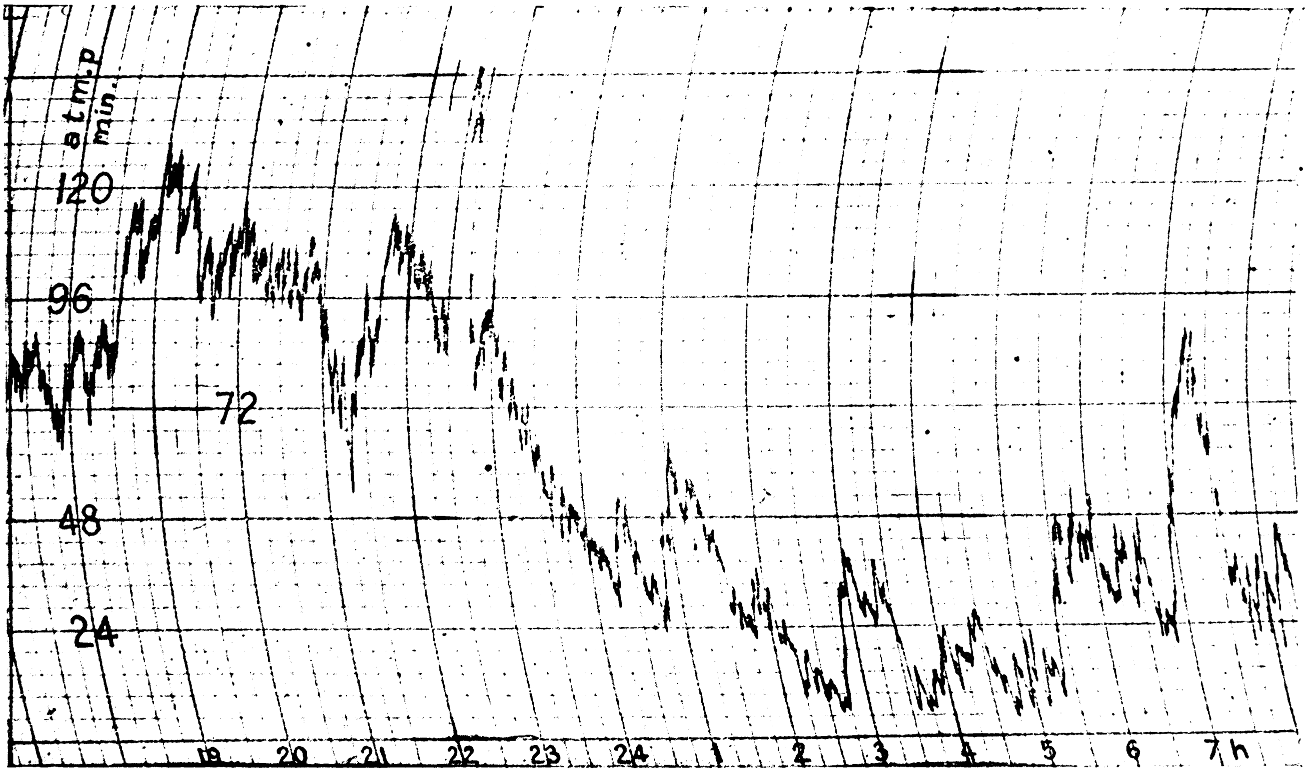

Fig. 2.

Exemple de courbe fournie par un récepteur enregistrant le nombre d'atmosphériques par unité de temps. — Enregistrement à Paris (Mont-Valérien) du 21 septembre 1927 à 17 heures au 22 septembre à 8 heures. — La montée brutale entre 22 h. 30 et 22 h. 40 n'est pas due aux atmosphériques, mais aux signaux horaires de la Tour Eiffel.

1. Appareil enregistrant le nombre d'atmosphériques dépassant une intensité déterminée.

Un récepteur spécialement établi pour donner une valeur aussi faible que possible du rapport signal/atmosphérique est placé en avant d'un relais qui fonctionne chaque fois que se produit un atmosphérique dépassant une intensité déterminée. Ce relais lance un courant dans un appareil normalement utilisé en météorologie pour enregistrer électriquement à distance les indications d'un anémomètre, l'anémocinémographe Richard. Dans cet anémocinémographe, la plume qui trace la courbe est sollicitée vers le bas par un mouvement d'horlogerie actionnant deux plateaux parallèles entre lesquels tourne une roulette dont l'axe porte une vis sans fin tournant dans un écrou. Ce premier mouvement tendrait à placer la roulette au centre des deux plateaux. Un second mouvement tend à l'éloigner de ce centre. Il est provoqué par les mouvements de bascule de l'armature d'un électro-aimant. Ces mouvements de bascule ont lieu chaque fois qu'un courant est lancé dans l'électro-aimant. L'appareil est construit de telle manière que la hauteur

de la plume est proportionnelle à la vitesse avec laquelle se succèdent les basculages du relais de l'électro-aimant. Dans le cas qui nous occupe, l'ordonnée de la courbe sera donc proportionnelle au nombre d'atmosphériques apparaissant pendant un intervalle de temps déterminé.

2. Appareil enregistrant la quantité d'électricité produite par les atmosphériques dans un appareil récepteur. (Cet appareil est basé sur un principe exposé par CURTISS.) A la suite d'un récepteur établi pour donner une valeur aussi faible que possible du rapport signal/atmosphérique, on place une lampe rectificatrice (triode dans laquelle la grille est rendue fortement positive de manière à réduire la résistance intérieure); on amplifie par une seconde triode le courant continu fourni par la lampe rectificatrice; on charge un condensateur à l'aide de ce courant continu. On laisse fuir le condensateur à travers une résistance et l'on enregistre ce courant de fuite dans un milliampéremètre enregistreur.

Les figures 2 et 3 donnent un exemple des enregistrements fournis par les deux appareils décrits ci-dessus. Les enregistrements ont été faits le même jour en utilisant le même appareil récepteur. Ils montrent que, dans l'ensemble, les deux courbes ont la même allure avec cependant quelques différences qui proviennent du fait qu'à certains moments il existe des atmosphériques rares et forts et à d'autres des atmosphériques nombreux mais assez faibles.

3. Radiogoniomètre instantané des atmosphériques. (Cet appareil n'a pas encore été essayé.) Deux récepteurs dont on peut à chaque instant vérifier et rétablir l'identité sont branchés sur deux cadres solidaires à angle droit l'un de l'autre. Les courants de sortie des deux récepteurs sont rectifiés et chaque courant continu charge un condensateur. On relie à travers un milliampéremètre ou à travers un galvanomètre les deux condensateurs. Si on relie les armatures de même signe, le courant dans le milliampéremètre sera maximum (au cours de la rotation de l'ensemble des deux cadres) au moment où une source d'atmosphériques sera dans le plan de l'un des cadres. On pourra ainsi séparer éventuellement plusieurs

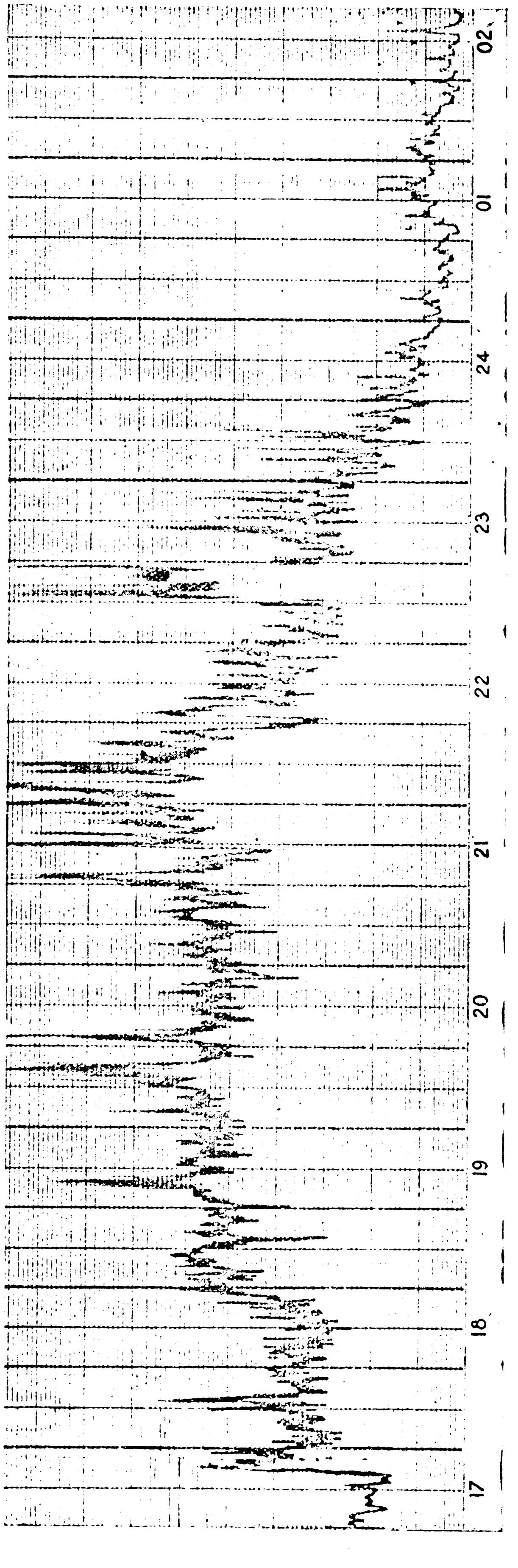

FIG. 3. — Exemple de courbe fournie par un récepteur enregistrant la quantité d'électricité produite par les atmosphériques. — Enregistrement à Paris (Mont-Valérien) du 21 septembre 1927 à 17 heures au 22 septembre à 2 heures. — La montée brutale entre 22 h. 30 et 22 h. 40 n'est pas due aux atmosphériques, mais aux signaux horaires de la Tour Eiffel.

sources d'atmosphériques se trouvant dans différentes directions.

Cet appareil a l'avantage d'opérer sur des atmosphériques individuels. Il évite de déterminer des directions en opérant successivement sur des atmosphériques différents.

Prof. Mesny. — It would be interesting to compare methods of recording in order to see how they differ in results. Dr. Austin, have you any records over a long time?

Dr. Austin. — No, we have a recording integrating device which gives us the integrated strength of the atmospherics but not in regard to direction. That we are just beginning now. We have taken a great many observations with different types of receiving apparatus, that which gives the instantaneous intensity and that which gives an integrated indication, but that has all been done by observation. We have not had a recorder up to this time which told us anything in regard to the direction. We have, however, some records of the strength regardless of direction.

Dr. Appleton. — Mr. Watson Watt has made a new arrangement whereby the broadcasting companies have cooperated in a very simple experiment of giving quite a number of trained observers a typewritten copy of a particular talk that is to be given on a particular night, and these observers have the typewritten talk in front of them and they listen with their earphones on and they make a mark when they hear a particular atmospheric. The atmospheric may come, say, between two words or between two syllables, but as it is recorded by quite a number of observers, the results are astounding. We discovered that sometimes 50 % of these observers, some of whom are 2,000 miles apart, may hear the same atmospheric. From these experiments we have the undoubted result that atmospherics which are recognized in that way can be simultaneous over distances as great as 2,000 miles from their source. This fits in with experimental results obtained in America and in Germany. The apparatus for the method I have mentioned costs nothing. It simply requires the cooperation of broadcasting companies. No doubt it could be imitated very easily in other countries.

Captain Bureau. — The results mentioned by Dr. Appleton were participated in by myself also, and the results obtained were partly in accordance with those which he has obtained and partly in disagreement. We should remember that atmospherics are very complicated and the kind of apparatus employed will bring out one particular item of atmospherics or another. Many atmospherics are continual reverberations which do not lend themselves to exact indication. In one set of observations in the neighborhood of Paris the apparently observed atmospherics turned out to be due to disturbances from electrical machinery; so we must use great judgment in interpreting the results obtainable from any particular apparatus.

Listening sometimes to the Daventry station a number of atmospherics were noted, and when Daventry ceased sending and the tuning was changed the atmospherics which had been previously observed disappeared. There seemed to be some relation between the wave length of Daventry and the particular set of atmospherics received. I suggest the possibility that the atmospheric disturbances in the neighborhood of Daventry may have in some fashion mutilated the waves issuing from that station and the appearance or disappearance of atmospherics is often attributable to the particular wave length on which the measurement is made.

Dr. Appleton. — Another explanation is that when Daventry was sending you were receiving the carrier wave from Daventry and that made your set sensitive and when Daventry stopped it was reduced.

Captain Bureau. — It would be desirable to repeat the experiment.

Dr. Appleton. — Using the ordinary broadcast receiving set on which I listen and which is extremely insensitive, I find that in every case that I receive one of the clicks which Dr. Austin has spoken of, there has been a lightning flash within three miles from my own house. I have never heard a crash, in the course of five or six years of observations, that could not be so explained.

Mr. Friis. —We record the average of the intensity of atmospherics received during a fifteen seconds period and we have found that satisfactory. Atmospherics are received on a rotating loop antenna combination. The recording apparatus was described in a paper published in the Bell Telephone Journal one and one-half years ago and I would be glad to send a copy to anyone interested.

Prof. Mesny. — It seems to me that it would be better not to have general observations every day but to choose some days a week for general observations because if we have observations every day it would be quite impossible to carry out the program. Has the photographic method been tried out?

Mr. Alexanderson. — The method of transmitting telegrams photographically has not yet been put in commercial service, but the apparatus is being developed and we have had enough experience with the apparatus on the stations to know that it can be done well. Similar work has been done in Germany, England and France, in fact all over the world; so difficulty over having suitable recording apparatus does not exist.

Dr. Appleton. — The apparatus is now working very well between England and Scotland and the results are being taken every day. The idea was conceived by Mr. Watson Watt as early as 1916 and he has had to put it aside for various reasons and within the last two years he has developed it so that it gives accurate results. I cannot give you any more information than that.

Prof. Mesny. — Has anyone any remarks to make about recording observations?

Captain Bureau. — I wish to point out that a method may sometimes be used which is not expensive and which is capable of giving satisfactory results. I refer to the system of airdrome radio distribution in France. At certain hours of the day observations are taken of the signals which are received at fixed hours from certain stations of fixed intensity; and records are made as to whether the signals are disturbed by atmospherics or not. In that way it is possible to obtain for the whole of France statistics indicating very clearly whether atmospheric disturbances are found for certain days or not. On certain days there are hardly any disturbances at all and on other days there are a great number of disturbances, and it is very evident from these observations that the number of disturbances from day to day may vary enormously, from 0 to 500. Thus such observations are possible, and are capable of giving valuable information. The precision of observation, although small, is far superior to the precision of the phenomena under observation.

Prof. Mesny. — I think we could go to another part of our program and discuss the frequency distribution of interference. That is a question which is very much discussed and very important. I remember some discussion between Dr. Austin and Mr. Pickard. Perhaps we could begin this discussion again now with more definite results than at that time.

Dr. Austin. — I suppose you refer to our discussion of the question whether atmospherics were aperiodic or whether they were strongly damped wave trains. We have a great number of observations on that point. There is no universal rule in regard to the change of strength of atmospherics with wave length. We can only say in a very broad way that there is apparently a very rapid rise in the strength of the atmospherics generally between about 8,000 and 12,000 meters. Of course there is a general rise all over as a rule though at times we have just as heavy atmospherics, especially at night, on the shorter wave lengths as we have on the longer. Leaving out of account the ultra-short wave lengths about which I know very little, it appears that on the whole the atmospherics increase in intensity with the wave length in the day-time and that there is a particularly rapid rise frequently in the region of a wave length of 10,000 meters. From 13,000 or 14,000 meters up to 20,000 meters, the change very frequently is not very great. Of course these statements must be very general. These rules are not always true by any means, but that in a broad way is our impression in regard to the changes in intensity of atmospherics.

The sensitiveness of the receiver at the different wave lengths is very well known. There is no question about that because we have to keep a calibration of our receivers for the measurements on the different stations at the different wave lengths so everything is really given on a common scale.

Dr. Hulburt. — We may regard the cloud discharges as a condenser discharging through a resistance, giving low frequencies.

Dr. Appleton. — We can get a very good analogy between the problem of receiving continuous wave station in the presence of atmospherics with looking through a spectrosope at a spectrum of white light with a sodium line superimposed. We can regard the problem of white light as being similar to the problem of atmospherics. We have a whole series of pulses of no definite wave length reaching our spectroscope and superimposed on that we have the sodium wave length and whether we can separate these things depends a good deal on the resolving power of the instrument. Therefore, if we follow our analogy we are interested in the energy spectrum of these pulses, and it is of interest that if we take any form of a pulse every one of these pulses that we take gives an energy spectrum which is exactly similar to Dr. Austin's curves. That particular point of investigation really gives us some information about the width of the pulse. There is some definite connection between the thickness of the most frequent pulse and Dr. Austin's results. These results tell us something about the thickness of the original pulse in the first case. Another factor we must remember. Two days ago we were talking about the function of the atmosphere in transmission problems and if, as I believe, these atmospherics travel a long way we must recognize that the atmosphere treats all of the wave lengths differently. I am particularly anxious to know whether this curve alters by day and by night. We should imagine for short waves in the case where signals are better by day than by night we should expect any distant atmospherics to be better by day than by night. Next the problem leads us to discuss the thickness of the pulse and is it really the same thickness as the pictures that we get on the oscillograph. In many cases I think it is but in other cases I think that the pulse which causes the damage is really what we call the high frequency ripples on the top of the main wave. Mr. Watson Watt found when he took the apparatus to the Tropics that these pulses we get showing the wave forms were very selective.

Mr. Pickard. — Prof. Appleton has just pointed out that our atmosphere treats different frequencies quite differently. Is it not also possible that our atmosphere may to some extent treat differently a pulse from a sustained train of waves? We now have some evidence that there is a difference. In measurements of the polarization of short waves I have found that the ratio of the horizontal to the vertical electric force was quite different for atmospherics than it was for a signal of the same frequency. I have also found that for atmospherics in the broadcasting band of frequencies, — at 1,330 kilocycles or 226 meters — that the correlation with the accompanying signals was generally inverse. Somewhat less definitely, the same inverse correlation with signal appears with the long wave length atmospherics which Dr. Austin has measured for so many years.

Dr. Austin. — I think you probably all know that these results which I have spoken of are only for daylight atmospherics and for long waves. At night conditions are entirely different and the short-wave atmospherics come in with great vigor just as the short-wave signals do.

Meeting of October 18, 1927

Prof. MESNY, Chairman.

Prof. Mesny. — The question for discussion to-day is the source and origin of atmospherics. Knowledge of the source is very important, not only to satisfy scientific curiosity, but also to determine whether sources of atmospherics are also the seat of special ionization phenomena. It seems likely that we shall find important correlations in the study of sources of atmospherics and the study of wave propagation. If we assume that atmospherics have great distance ranges, we may from a certain point of view conclude that some of the sources are in the very high regions of the atmosphere; because it is known that often there are many strong atmospherics in very wide areas over which the sky is perfectly clear.

I think we must consider the question in the following manner. Some think that the source of atmospherics is very far distant, and they put it in tropical regions on the continents which are most heated by the sun; others consider that sources of atmospherics are comparatively near, not farther than some tens of kilometers away from the place of observation. It is possible to understand these two ideas, and our program to-day is to consider the different arguments as to the distance range of atmospherics.

Je tiens à faire ici une remarque relative à la méthode par laquelle on détermine la position d'une source d'atmosphérique au moyen d'observations radiogoniométriques. Si l'on suppose qu'il existe un grand nombre de centres distribués régulièrement le long d'une ligne les observations radiogoniométriques en un point donneront la direction de la normale à cette ligne, et des observations faites en différents points fourniront par leur recoupement le centre de courbure moyenne de la ligne, point qui n'est évidemment pas le lieu d'origine des phénomènes. Or dans certains cas, il est certain que les sources sont distribuées sur de longues lignes; le capitaine Bureau a fait d'intéressantes observations sur les perturbations qui accompagnent le passage de lignes de grains.

Je demanderai maintenant à ce dernier d'exposer ses idées sur l'origine des atmosphériques; M. Appleton nous donnera ensuite son opinion.

Captain Bureau. — Atmospherics are produced by the same mechanism as produce thunder storms. I refer to local storms only. Atmospherics are produced at distances within about 50 miles from the station. There is clear correlation between atmospherics and meteorological conditions, of which therefore atmospherics may be a convenient mode of detection. Meteorological conditions are of course complicated, but atmospherics have their origin in meteorological conditions.

. . . .

(There followed a long discussion between Captain Bureau and Dr. Appleton on the origin of atmospherics, Captain Bureau claiming the atmospherics come from short distances, and Dr. Appleton that they come from a long distance.)

. . . .

Captain Bureau. — I have also made observations on faint atmospherics. In our meteorological service we are trying to get signals from America which are very faint in France and require much amplification. I have found that on these American signals on certain nights there are atmospherics and on other nights there are none, and on the nights where there are no atmospherics it could be correlated with a very definite meteorological condition. I can only show diagrams obtained by apparatus of low sensitivity, but the work has been done also with apparatus bringing in faint atmospherics.

Dr. Wilson. — We have found that these weak atmospherics in almost every case during the day-time in the summer appear to come from the southwest; except when there are nearby storms, in which cases we find they always come from the direction of the storms.

Dr. Taylor. — That is true for all points on the eastern North American coast, but on the west coast of America the major portion of the atmospherics come from straight inland towards the mountains. In Honolulu they come from all over.

Captain Bureau. — In Algeria the atmospherics usually come from the mountains region which is roughly parallel to the coast extending from east to west.

This particular kind of atmospherics begins in the morning and has a maximum at 3 o'clock P. M., and goes down thereafter. This is typical of summer conditions when the weather is very warm and the ground is heated by the sun. This is sometimes conductive to actual storms but you have the atmospherics even in the cases where you do not have any storm. This kind of atmospherics is the same in quite a large area of the country. This tends to explain how in certain geographical conditions the atmospherics seem to come from the high and flat regions. In those regions the air is clearer and less dense. In regions of high mountains, the direction of atmospherics has sometimes been observed to turn in the same way as the sun because then the direction of the place which is heated by the sun rotates in that direction.

Dr. Appleton. — How does Captain Bureau explain that as you sail across the Atlantic atmospherics come from Africa and as you get nearer America they come from America?

Captain Bureau. — I have tried to find an explanation of the phenomenon, but have not been able to find a satisfactory one. It seems to me that the phenomenon is contradictory to the fact that the intensity of the atmospherics is different from one day to another on the ocean. Both facts are established by numerous observations, so there cannot be a contradiction. Certainly there is a variation in the number of atmospherics as you cross the Atlantic. When I crossed the last time I was fortunate enough to meet two storms. I found that the number of atmospherics that I heard checks very nicely with my theories of the meteorological conditions which bring about a building up or sloping down of the curve of the number of atmospherics.

Dr. Appleton. — Everybody agrees that there are local meteorological conditions which are linked with certain types of atmospherics. There is no doubt about that. But the atmospherics which cause the trouble to the radio engineer, do they originate within 50 kilometers of the station or farther away?

Captain Bureau. — My observational data give the answer to that. The curves which I have shown you are very recent and only date from October last, but I have been taking data over several years. These observations have been exclusively from the point of view of actual radio reception. I am in charge of the communication side of the Meteorological Service and therefore have been interested in plotting on the maps every day the stations which my operators receive. I have been interested in finding a correlation between groups of stations the reception of which were troubled with atmospherics, both the strong and the faint ones. In this way I have built up my theory of the atmospherics in terms of local meteorological conditions. My opinion is that all atmospheric interference that interests the radio operator is caused exclusively by conditions less than 50 kilometers away, the only exception being the distinct clicks which come from far away; how far it would be difficult to determine, but as these clicks are not continuous they can cause no serious trouble to the operator. All others originate less than 50 kilometers away.

Mr. Yokoyama. — Our opinions in Japan are exactly the opposite; we find that the more troublesome atmospherics come from a great distance.

Captain Bureau. — I do not think that this is contradictory with my point of view, because if you admit my theory then you would not take the concurrence of observations of direction to mean that the atmospherics originate from that point but that all these directions happen to be normal to the more or less curved boundary of a whole region.

Dr. Appleton. — Observations of atmospherics in England, lasting over some years, indicate that both near and far atmospherics arise from thunder storms. Our work arose in connection with the problem of accounting for the earth's electric charge, and an attempt has been made to find out how many thunder storms there are in action at any one time over the whole world, and whether the thunder storm areas coincided with the areas which are prevalent sources of atmospherics. The agreement is remarkable. When maps are prepared it is found that there are some places which have a storm every day and these great thunder storm areas are East Indies, Africa, and a region in America around about the Gulf of Mexico. If in general apart from just local storms, if the source of atmospherics are these great thunder storm centers, then the source of atmospherics should follow the sun because we know that at any given place the intensity of thunder storms reaches its maximum in the afternoon and then it is gone and is at a minimum at night. As the earth goes around the sun we find that at each place where you have 4 or 5 P. M. it would be at its maximum as the source of atmospherics whereas at night when the problem is complicated by better transmission conditions you would expect atmospherics to come from all kinds of places. That is what happens. Very careful measurements have been made by Mr. Watson Watt and he has found definite correlation with direction and time of day. In the early morning the atmospherics appear to come from the east and as the sun comes along the direction alters and follows the sun. At night, of course, they are left in the west. That idea of the thunder storms producing atmospherics really originated in connection with studies of the earth's negative charge. Now the picture of the atmosphere we have is as follows : we think of the thunder storm belt around the equator as the main source, but local thunder storms can produce both clicks and grinders. To turn back to the linking of the earth's negative charge with the thunder storms we now picture thunder storms as always being set up in one direction having a positive charge on top and a negative charge underneath, and keep the balance of the ordinary air earth current for which previously there has been no definite reason. The positive charge at the top of the cloud gets discharged in the upper atmosphere which might be very potent as an interference agent. Mr. Watson Watt, working in the tropics, has found that seems to occur. On occasions he has noticed a discharge from the top of a cloud which has very potent interference. It seems to me we are talking about two different things; Captain Bureau is talking about the local action, and we believe they represent quite a small portion. They do not occur every night of the year. I cannot remember a night when with a powerful receiving set I have heard no atmospherics, and I feel that that observation leads one to suspect that his set was not sensitive but whether the operator can receive a signal or not depends on the signal-to-static ratio. I think the static recorder of Mr. Friis is the best apparatus for finding out what we want to know. Observations on radio reception are dangerous because it is merely a question of whether you can receive your station. Probably someone here could tell whether their atmospherics come from those three regions which are the definite regions of thunder storms.

Prof. Yokoyama. — Your observations were very interesting because they fit in with our explanation that for us the East Indies are the principal source of atmospherics. In my experiments the grinders come from the East Indies and the clicks are due to local origin.

Captain Bureau. — I did not say that all clicks come from far away.

Prof. Vanni. — I have an article by Colonel Sacco of the Italian Army which I would be glad to show to anyone interested. Objection is made to the use of the radio direction finders for observations of atmospherics. They require a certain number of seconds to make an observation whereas an atmospheric is a very rapid phenomena. The question is raised what would in the opinion of the persons present be the best method to recommend to find the actual direction of the atmospherics. In another part of the paper Colonel Sacco wants to know what probability there is of the various directions, found in different places, where they converge into the actual center where the atmospherics originate from, and finds that this probability would be very small. I wish mainly to point out these conclusions of this paper and to draw the attention of the members to this paper which seems to support the remarks made by Captain Burean, and also to ask what the best methods would be suggested to find the actual directions where the atmospherics come from on a given point.

Dr. Appleton. — English methods are not subject to such criticism because they are obtained by a coil which is rotated by clock work and use a very rapid indicating instrument. The difference between the north and south direction is very marked. The apparatus of Mr. Watson Watt gives a statistical average of the direction in which the atmospherics are the most numerous. It uses two loop antennas at right angles and a cathode-ray oscillograph. Two such observing stations about 1,200 miles apart are used simultaneously. They are connected by telephone and are able to tell one another of the appearance of each atmospheric. They find that atmospherics originate in various parts of Europe. The extraordinary thing is that when the weather records come from all over Europe you find that the atmospherics are dotted on the records at the places where thunder has been reported. Records have been published by Baumler, giving cases where simultaneously in Germany and in America on the four dots of the letter H the same atmospheric was recorded, just at the same spot between two dots of the letter H thereby proving that an atmospheric can travel that distance.

Captain Bureau. — The method you have shown indicates atmospherics that come from Europe and not the tropical countries that you mentioned.

Dr. Appleton. — Normal atmospherics come from the same direction, north and south; it is not possible to get a base wide enough in England to separate distant and local sources.

Captain Bureau. — Hubert has observed two kinds of atmospherics, one due to extremely strong tornadoes which do not reach over 200 kilometers, and a second kind which are only local. It seems difficult to understand how if the origin of the atmospherics were in equatorial regions they could be so strong as to influence reception every night at such a distant point as England.

Meeting adjourned at 6 : 20 P. M.

Meeting of October 20, 1927

Prof. MESNY, Chairman.

Prof. Mesny. — At our last meeting we had a very interesting discussion between Dr. Appleton and Captain Bureau, who presented the cases for the long range and short range atmospherics. We did not come to any conclusion, and whether we would have come to one if the meeting had lasted longer is doubtful, but perhaps a few specific points have been cleared up. I said the other day that it did not seem to me that the convergence of the directions found with the direction finding apparatus were a proof that the static originated from some distant point, as shown on the map, because if the directions that were found at each point were the normals to a certain curve then they would converge on a certain center though for that reason not being necessarily created in that center. Dr. Appleton's answer to that was that the observation had been made with unidirectional direction finder and therefore according as to whether he was inside or outside of the circle he would find diverging or converging lines.

Je pense que les sources d'atmosphériques, au lieu d'être réparties suivant des lignes de discontinuité, comme dans le cas des lignes de grains, peuvent s'étendre sur des surfaces très étendues, celle d'un océan ou d'un continent tout entier par exemple. Dès lors, ce que l'on déterminerait avec le radiogoniomètre ce serait les centres de courbure des lignes d'égale activité de perturbations, et comme on mesurerait en quelque sorte le gradient de cette activité, le radiogoniomètre donnerait bien toujours la même direction si l'on supposait ce gradient toujours dirigé vers une même région. Cette hypothèse est très vraisemblable, cependant si l'on cherche à la traduire en chiffres on est conduit à des valeurs bien fortes pour l'activité des perturbations aux points où elles sont les plus fortes.

I do not want to insist much on this question as quite certainly it is only by numerical measurements made in order to test the various hypotheses that some light might be thrown on the subject, and it is probably not in the very near future that it will be possible to get this kind of numerical results.

Dr. Van Der Pol. — The experiments of Baumler and Goldsmith have not been discussed at any length. I think this point has been settled rather definitely by these experiments of Baumler and Goldsmith. I may perhaps remind you how these were done. In Berlin and in New York the time signals from Eiffel Tower were received and written down by some recorder and on the top of these time signals, one found in Berlin and here in New York, simple irregularities due to atmospherics and I remember well I think that some 80 % could be correlated and could be found to happen practically simultaneously in Berlin and New York. If these experiments can be relied upon perhaps it would be interesting to have them repeated again. I think these experiments give a definite proof that simultaneous atmospherics occur at places which are far away on the earth and this looks to me as being the proof that they originate from some big distance or at least they propagate over a big distance.

Baumler states that when it was certainly quite calm there are no atmospherics in Berlin and this happened to be the case on the day there was a train strike in Berlin so lots of atmospherics appeared to come from the tramway in Berlin. You must be well away from electrical machines when making records of this kind.

Prof. Mesny. — I entirely share the interest of Dr. Van Der Pol in Baumler's article, and I am sorry not to have been able to study Baumler's paper here as I intended to do before this meeting. If I remember right, Baumler on the one hand, eliminated a certain number of atmospherics before taking his percentage and on the other hand he assumes that two notches that brought out the same relative place on the dents of these time signals referred to the same atmospheric, and in his experience the atmospherics always, if I remember, were very numerous. Now if you distribute on a small straight line a great number of little notches then there is certainly quite a good probability that a number of them will fall out about the same points especially if we consider that the measurement is not extremely accurate. That would be a matter of using the calculus of probability and finding what the correlation factor would be. I do not wish at all to give these remarks as a contribution to the problem; as I said in the beginning, I did not have time to study Baumler's paper before coming here.

Dr. Austin. — There seems to be plenty of arguments which can be advanced for both sides in regard to the distant and nearby atmospheric origin. I do not think there is any question, that is, there is no question in my own mind, that we have a great deal of atmospherics produced practically everywhere on the earth. We certainly have a great deal here in our neighborhood because the correlation which exists between local thunder storms (200 miles) and atmospherics during all the summer months is very high. There is also a high correlation existing between local temperature and the atmospherics, but on the other hand there are all the experiments which indicate the simultaneous occurrence of atmospherics at very distant points, suggesting a common distant origin. Some of Baumler's more recent work shows simultaneous atmospherics in Berlin and Honolulu. These must be on nearly opposite sides of the earth. In addition to this, we can trace tropical atmospherics as they follow the sun. With our direction finders we can trace the march of atmospherics as the sun goes north. In winter it comes from east of south from South America or even perhaps Africa, and then in spring it goes up across the Isthmus of Panama and Central America into the regions where the land areas lie well to the west of us. I think there is therefore no reason for a difference of opinion in regard to this matter. I do not think we can say that all of the atmospherics are produced nearby or all produced at a distance. The point for argument would be, perhaps, the percentage of atmospherics at a certain season of the year which is due to comparatively local conditions. I think undoubtedly both Captain Bureau and Dr. Appleton are quite correct in their measurements but they are not always measuring the same thing. The question of whether you can distinguish between the types of atmospherics which are mainly produced nearby and those which come from a distance is an interesting one which I do not think we are able to settle at present. If we assume that our so-called « grinders » are the summation of a large number of short discharges which correspond to our clicks we would naturally suppose that the clicks would be most prominent in the nearby disturbances because there we are not integrating from such a great number of sources, while if we are listening to all the atmospherics of the tropics we would almost certainly have the separate clicks united very largely so as to form something like « grinders ». Of course it is possible that we are dealing with entirely different types of discharge, that grinders are really different from the clicks, but it is quite generally suspected now that after all the grinders may be simply a combination of the clicks which come so close together that we cannot separate them.

Prof. Mesny. — I agree with Dr. Austin and I think that most everyone, including Captain Bureau, thinks that there are local atmospherics and distant atmospherics. However, the question which Dr. Austin has asked as to which is the local and which the distant atmospherics, has been put in discussion several times in our last meeting, and Dr. Appleton and Captain Bureau both agreed that what they were interested in was to determine the origin of the form of atmospherics that was most troublesome in the traffic and both agreed that these disturbances were the grinders, at least in our region, because the clicks were isolated and less frequent and really not so troublesome as the grinders, but the argument stopped there, and they both very definitely asserted their opinion, Dr. Appleton that the grinders were of very distant origin, and Captain Bureau that they come from a 50 kilometer region.

I would like to mention another view which has been put forth by Mr. Bellescize of France. Mr. Bellescize thinks that we would have to assume much too big a discharge to account for a really distant atmospheric but suggests that it might be possible that on quite a great distance the atmosphere would be so unstable that a small discharge at a given point could start another discharge at a small distance in the manner that a match sets fire to a string of powder, and it might be possible that among the several possible physical phenomena that might take place, this might be one of them.

Dr. Austin. — During the winter when the sun is far to the south, of course our atmospherics become very weak, and if we consider the motion of the sun from south to north between winter and summer the difference in its angular distance to Washington would come very close to explaining the difference in intensity of the atmospherics which we measure in mid-winter and in mid-summer, not exactly but with an error not greater than 2 to 1 or something of that sort, and if you consider that with the same apparatus we are measuring signals on stations in Buenos Aires, for instance, I do not see why it should be difficult to conceive of atmospherics which would be powerful enough to come to us from the same region since most people feel that atmospheric discharges often are comparable to or stronger than any single station that we have.

Prof. Van Der Pol. — There is another argument favorable to regarding atmospherics as coming from a great distance. Suppose atmospherics to be caused by lightning flashes. One can make an estimate of the number of lightning flashes every minute on the earth. There are so many thunder storms a year, they last so long, and in each thunder storm there are so many flashes. At a certain point on the earth there is a thunder storm from such an area and a summary can be made of the number of thunder storms on the earth at that time and the number of flashes which will occur on the earth every second. The number of lightning flashes so calculated is sufficient to account for the number of atmospherics observed.

Prof. Mesny. — I would like to point out a fact of observation, which is that we may all have observed actual thunder storms existing at rather short distances (50 or 60 kilometers) at a time when we had no atmospherics at all in our receiving sets. On this question of the distance at which a thunder storm is heard, I think Dr. Austin had doubts on the matter in his article where he reported atmospherics as coming from the Gulf of Mexico. He had made some calculation as to the energy that ought to have been present in the discharge and which he found to be very much bigger than was likely to be expected, especially in view of the fact that in our stations we have sustained waves to which we can tune, whereas the atmospherics are aperiodic.

Dr. Austin. — I do not remember about those calculations. Of course it is true that we can have thunder storms at no very great distance from us which produce very little effect on our measurements. In regard to that I think we must consider that there is probably a great difference in thunder storms and lightning discharges. Mr. Watson Watt has found that in the thunder storms in the tropics the curves of his lightning discharges in a very great number of cases, show these ripples on the main curve of the discharge and to me it has always seemed reasonable to believe that these ripples were the effective portion of the discharge. It is still a matter of some discussion whether a distance of the form of the main curve of Mr. Watson Watt would give us the effects which we know we have from the atmospherics, but I think there is no question that these ripples, where they exist, would very easily explain atmospherics. So if it is true that the thunder storms in the northern latitudes are freer from these ripples it is quite easy to understand how our thunder storms are less effective than those in the tropics. Now if we consider the tropical land regions where there are a great number of discharges taking place simultaneously in different thunder storms, we do not then have really the conditions of radiation from a central point. If these flashes are occurring simultaneously as they probably often are, where there are so many storms over a wide region, we should have to a certain extent radiation from a line rather than from a point which would tend to increase the intensity which we would observe at any given distance. Just how much that last argument amounts to, I don't know because we should have to know to begin with just what degree of simultaneous occurrence we could expect in the lightning flashes and just what their probable phase relations would be when they reach us.

Prof. Mesny. — It is really a pity that Dr. Appleton is not here because I would have liked to have asked him something along this line. The oscillograph lines that have been observed by Mr. Watson Watt have been observed with a Western Electric oscillograph and observed by the eye, and a line has been drawn from which Mr. Watson Watt has deduced the frequency of the phenomena and its maximum was, I think, 10,000 or something of the sort. It is impossible if the frequency were of the order of a few thousands of kilocycles that the method employed would have detected them. That was a very serious argument against the method and I would have liked very much to have the opinion of Dr. Appleton on this point.

Dr. Austin. — Of course I have considered this question. It seems like a serious difficulty. They are certainly difficult measurements as is shown by the fact that no one has duplicated them.

Dr. Van Der Pol. — I talked about this to Dr. Appleton some time ago. Mr. Eckersley announced that he was of the opinion that only the ripples on the general wave form could practically be heard in the receiver and that the general form cannot be heard on the receiver, and I think that from the general analysis which Eckersley gives, and I am quite sure that Dr. Appleton agrees, that it is mostly these ripples that are audible. It depends on how sharp the curve starts. The amplitude of this frequency depends on the way this starts, but the general idea of Dr. Appleton is that it is mostly the ripples that give this.

Dr. Dellinger. — I have seen Mr. Watson Watt's apparatus and it seems to me that it is quite capable of determining these wave forms without error. While it may be true that the ripples may give the largest component of disturbance at the frequency at which we observed, nevertheless even the main wave of the form must contribute powerfully. A single pulse itself is capable of creating considerable disturbance in a receiving set at any radio frequency.

Prof. Mesny. — My objection was not as to the apparatus used but rather as to the method, but as we all remember, he saw the line flashing on the plate and simultaneously made the curve, and it seems difficult that in such a condition he could record very much outside the general form of the signal.

Dr. Austin. — It is quite possible to see the ripples because I have seen them myself. As to following the exact shape that is another question. I do not think that anyone could draw the ripples with accuracy so as to tell exactly their spacing but you could do it in a rough way and they at least indicate that you had something of a very much higher frequency than the main pulse.

Mr. Pickard. — It has been suggested that there was a difference between the character of lightning discharge in the temperate zones and in the tropics. It is possible that we might established some difference, possibly in the length of the path as between the tropics and the temperate zone.

Captain Bureau. — There are two different kinds of storms which do exist both in the tropical region and in the temperate region and the first kind is due to the movements of the hot air that is heated by the ground and this kind of movement makes a storm cloud which in the temperate region usually goes as high as 1,000 meters. In certain conditions it might develop as high as 9,000 or 10,000 meters. The upper layer of the cloud might be as high as that. Whereas in the tropical region as a rule they develop as high as 9,000 or 10,000 meters. The second kind of storm is not local like this but is due to a cold wave coming and shifting itself under a hot body of air and causing instability. But this thing is not local but gives a line of instability on which there are storms. The first kind is rather the summer kind and the second kind the winter kind, and they exist in both the tropical and temperate regions. The second kind has been studied closely by Hubert in Equatorial Africa and he has found that the atmospherics due to those storms were not heard at a distance farther than 150 miles from the place where the storm was.

I agree that there seem to be various kinds of storms from the point of view of the action of radio receivers and that I myself have noticed conditions in which you had atmospherics without storms in the meteorological sense of the word, and also meteorological storms and no atmospherics, but I think that both the storm and atmospherics depend on the local meteorological condition and are different aspects and independent conditions, and they are, so to speak, brothers but not father and son. They develop from the same cause, but one might be bigger than the other and vice versa.

Dr. Austin. — I understand that Captain Bureau does not consider that lightning is the only cause of atmospherics.

Captain Bureau. — Only a small part of the atmospherics is caused by lightning.

Dr. Austin. — Of course there may be other discharges in the thunder storm besides the visible lightning.

Prof. Mesny. — I have received a letter from Dr. Appleton who regrets very much, as we all do, that he has been obliged to leave u so soon and was not able to attend the other meetings. Dr. Appleton makes various remarks about the proposed scheme of observation on atmospherics suggested by the Royal Meteorological Society of England and he makes a few modifications to the program. I will read it and we will see if we have time to discuss the scheme now or if we want to postpone the discussion to some other meeting. This program was circulated some two months ago. Dr. Appleton put the subjects presented by the British Committee in a slightly different order. It is in the order of importance that he attributes to them. The subjects are continuous directional recording; synchronous observations on time of incidence; telegraphone records; synchronous observations of direction of arrival; synchronous observations of wave form. The subject that Dr. Appleton thinks most urgent and particularly interesting is directional recording. This would include the installation by each country of at least one of the recorders used by Mr. Watson Watt, which would be difficult.

The second suggestion is the synchronous observations on time of incidence. Scheme proposed by General Ferrié simply consists in recording all the time signals having records sent to some central bureau to see whether there would be some atmospherics appearing at the same moment.

The third proposition in order of interest, is called telegraphone records. It is proposed that each country within signalling range of a broadcasting station should record by telegraph and send to a central bureau. The telegraphone is an apparatus in which an iron wire moves between the poles of an electromagnet so that the incoming signals are recorded magnetically on the iron wire. The iron wire may be made to reproduce the sound again. You could send such wires to a central office for comparison and recording.

The fourth proposition is synchronous observations of direction of arrival. Some kind of directional recording is first made synchronously by different nations.

No. 5 is synchronous observations of wave form.

Before we start studying these various items I would like to ask whether any member has any further observation to suggest to add to this list.

Captain Bureau. — I would like to have the activity of atmospherics recorded. By that I mean not the intensity or the number but something like the integrated effect that would take into account both the number and the intensity. If at a given point of the country you have at the same time atmospherics showing a directional quality and atmospherics which do not, then the directional method will select those which have a directional quality and will give directions on the map converging into a constant point. If there is a zone from which the atmospherics come then we ought to find the lines of equal intensity, being roughly circles around a point. But if, as I think, there is no such zone then the lines of intensity should give an entirely different pattern. I think that if some such plan of observations is not followed the divergence of results will continue between the methods that show directional effects and those that do not. It would be desirable to have the two things at the same time. I agree that it would not be easy to get exactly comparable results from different stations. Still I think it can be done and the apparatus to obtain such a result is certainly not more complicated or more costly than Mr. Watson Watt's apparatus, which in fact could be used for part of the work which I have mentioned.

Prof. Mesny. — On most of these questions we will probably make recommendations to the various National Committees.

Meeting of October 27, 1927

Prof. MESNY, Chairman.

Prof. Mesny. — At the last meeting we terminated the discussion regarding the theory of atmospherics. I indicated that I received a letter from Dr. Appleton relative to international experiments. The British Committee had prepared a program proposing experiments they thought should be made and Dr. Appleton's letter proposed certain changes in the order of such experiments. We discussed this in a general way. I will now take up the specific items, to see what we might do on international observations. The first proposal is as follows :

« 1. *Continuous Directional Recording.* — Each constituent country to be asked to install at least one continuous directional recorder, of the pattern described in the paper on « Directional Recording », per hundred thousand square miles of its area, with a minimum of one and a maximum of six instruments per country, the instruments to be as nearly as possible identical in construction, properties and adjustments. It is proposed that the records should be forwarded monthly to a central bureau charged with their reduction according to a uniform standard, with the return to each country of the reduced results, and with the comparison of all records. »

Wat we should consider is which countries can make these records and which countries cannot.

Dr. Austin. — We are already making records of the intensity but we shall soon be ready to make records of the direction, I think, within a week now. We are just completing our apparatus. The method of recording is somewhat different from that of Mr. Watson Watt in that we attempt to record the average intensity rather than the number of the pulses. It has not seemed to us that that was as valuable for our situation as the integrating method. We shall be glad to discuss the matter when we have had a little experience with our apparatus, and we would like to compare our results with Mr. Watson Watt's. I think in addition that the Navy has built or is constructing an instrument of the Watson Watt type which will be in operation somewhere in this part of the country, and we shall probably be able by comparing records on these two instruments to see which one is best suited to this region.

Prof. Mesny. — It seems that it will not be possible to have in each country just now records made with the same apparatus as that of Mr. Watson Watt and we will not have time to construct them for immediate use.

Dr. Austin. — I would like to ask Dr. Dye if he knows anything about the use of these recorders in the tropics.

Dr. Dye. — I am sorry that I have no information on that. I think it is better to leave the countries free to use their own apparatus. I do not think one is in a position to say that a particular type of apparatus is best for every country.

Captain Bureau. — Mr. Watson Watt believes that there are two apparatus of this type installed at Munich, Germany.

Dr. Austin. — I know that the Germans were quite determined to take his apparatus as standard but I think Mr. Watson Watt's idea in making this recommendation was very largely in order to get the work going. It has been dragging for a number of years. We have all said we were going to get apparatus for static recording and it has not been developed anywhere except in England and his idea was that as they had something developed, it would be a good plan to try that in the different countries. Now that we have something else I think we ought to make our experiments with the apparatus we have.

General Ferrié. — We will try to make some observations at the Observatory of Meudon and we hope we can do something. We will ask help from the private companies. We already have a recorder but not for direction.

Prof. Vanni. — Our position in Italy is the same as in France.

Dr. Austin. — I might say that our apparatus is identical with Mr. Watson Watt's as far as the dimensions of loop and rate of rotation, etc., are concerned. The only real difference is in the method of registration.

Prof. Yokoyama. — We are just beginning to record the atmospherics but not direction. I think it would be difficult for us to start direction observations now.

Dr. Austin. — Really the problem is in handling the data. You get such a vast quantity of material in such a short time that you need a central office to examine it.

Prof. Mesny. — The second proposition is : « *Synchronous Observations on Time of Incidence.* It is proposed that these observations should be made according to the scheme prepared by General Ferrié and his colleagues. »

There seems to be no difficulty about this part because the observations are to be made on the time signals. It would only be necessary in every country to record the time signals and the atmospherics will be recorded at the same time in the same way.

Dr. Austin. — We cannot as a rule record the time signals of the shorter wave stations of Europe here during the season of the heavy atmospherics. In fact, even in the case of Bordeaux, while we can hear it with the telephone, it would probably be often impossible to pick out the dots of the signals with certainty.

Prof. Mesny. — If not always, at least you could do it sometimes?

Dr. Austin. — Yes, we could do it sometimes. We would not be able to do anything across the ocean in the summer in the afternoon except on very powerful stations. I think the idea was to get the individual crashes.

Dr. Dye. — Yes, but the time signals make it easy.

Dr. Austin. — The time signals of the different countries are sent out at different times.

Dr. Dye. — You can take it from local time signal which would be accurate to one-half a second in case where you could not get the time signals that were sent out by the other stations.

General Ferrié. — I think it would be better to divide the question. First, in order to compare the static within a given region, we should have some signals for Europe only; many stations in Europe could record the time signals from one station, which could be, for instance, Eiffel Tower. In America they could do the same, and in Japan also.

Next, in order to compare the static in regions far apart, we could arrange signals which would be recorded only in a few points in Europe and America, perhaps only in Paris and Washington. In this respect it would be possible to record the time signals of Bordeaux or Annapolis. In Japan they would record the signals of Bordeaux.

Dr. Austin. — I am not sure that we would be able to record Annapolis and at the same time get a record of any atmospherics because apparatus sensitive enough for the atmospherics would be blocked by the signals from Annapolis here. The signals would be too strong for a station as close as Annapolis.

Prof. Mesny. — After this meeting we should prepare a letter to every country for their national committee in order that we might ask them to prepare for such work and to give their opinion about the question.

The third part is « *Telegraphone Records.* It is further proposed that as an optional and experimental item in the program, each country within signaling range of a selected broadcasting station should record, by the telegraphone, the transmission during one selected hour per week, and that these records should be compared at a central bureau. »

Captain Bureau. — It is impossible to have such experiments between different nations because it is very difficult for the Frenchman, for instance, to understand what is said in the English language. I think it is possible that sometimes the broadcasting is modulated by the atmospherics in the antenna and then when you hear an atmospheric at the same time as a certain signal the atmospheric comes from the antenna and we must record the same atmospheric at the same time. This is of course surprising because the power put in the antenna by the atmospherics is very weak in comparison with the power of the transmission.

Dr. Austin. — I am not sure that we would get very exact timing of our static pulses simply from the radio program, especially in the hands of inexperienced people.

Prof. Mesny. — It seems to me it would be a fantastic plan.

Dr. Austin. — I think recording is the only means by which we can get the timing of our pulses accurately enough to really draw any conclusions.

Prof. Mesny. — The question of reduction of the observations is very important. It is said here « It is estimated that the reduction of results from observations made according to this program would occupy the full time of two computers working under supervision. »

General Ferrié. — Before starting an international program it would be more simple to ask one of the national committees to study this question with another national committee. If we see there are interesting and important results we could try to extend it. To begin we could ask, perhaps, the British Committee to organize what experiments they wish with one of the national committees in Europe. It would be less expensive and we would see if there were any results. Then we could plan to make experiments internationally the world over. I think we should refer this to the British Committee.

Prof. Mesny. — Then we will ask the British Committee to organize a test with another national committee.

The fourth part is « *Synchronous Observations of Direction of Arrival.* It is further proposed that each constituent country should undertake to have made, at one or two observatories, observations of the apparent direction of arrival of individual atmospherics recorded in Q. 1, such observations to be made with the cathode-ray direction finder. The British National Committee undertake to prepare a standard specification for direction finding installations of this type, together with full instructions for observation. »

It seems that there is another difficulty there because the installation is more difficult. It is necessary to use complicated and delicate apparatus. It would be better to ask each committee to study the question and to make an installation if possible.

Dr. Austin. — Would the proposal be for one other country to make the experiments with the British Committee before we ask all the countries to do it?

Prof. Mesny. — Yes. The fifth part reads : « *Synchronous Observations of Wave Form.* It is proposed that each constituent country be requested to arrange that at the time of the synchronous observations (Part 2) there should be made at not less than one, and preferably at two observatories in each country, observations on the wave forms of such of the atmospherics recorded in Part 2 as can be delineated by the methods described in reference 2 « Nature of Atmospherics » — I). The British National Committee undertake to prepare full instructions for observations according to this method. »

I would comment the same as for the last question; the operation of the apparatus in that way can be discussed next year at the meeting from a theoretical point of view because there are doubts about the value of the results.

Dr. Austin. — It seems very doubtful whether the wave form as observed by Mr. Watson Watt is really static. It is a discharge but unless the rise in the curve is very steep it would apparently be impossible to produce the effects of static as we observe them. And if the ripples are really the cases of the static there is not very much object in putting a great deal of time on observations of the peculiar form of the main discharge.

General Ferrié. — We should make some studies so we can discuss this in next year's meeting.

Prof. Mesny. — Summarizing, we have two tests which can be made almost immediately, continuous directional recording, and synchronous observations on time signals. It would be necessary to correspond between nations to see the best manner of carrying on these tests. We should ask the National Committees to see what they will be able to do and what observations they can make.

Quant aux trois autres séries d'observations : enregistrement par télégraphone, observations synchrones des directions, formes des oscillations, les différentes nations ne sont pas prêtes à les exécuter et elles ont besoin d'examiner les moyens de les réaliser.

D'autre part, nos séances ont été entièrement chargées et je pense qu'il serait indispensable, pour la prochaine session de l'U. R. S. I. de ne proposer pour la discussion qu'un petit nombre de questions parfaitement définies; ces questions seraient présentées six mois à l'avance de façon à ce que chaque Comité national ait tout le temps de l'étudier.

Captain Bureau. — It seems to me that among the observations it should be possible to have an idea of the activity of atmospherics. I should like the observations to be made the same way all over the world, or at least at some points. Then we should find a way of indicating degree of intensity of atmospherics in order to be able to make charts of atmospherics. Certainly at first it would not be a perfect chart yet it would be very useful.

Dr. Austin. — We are doing that on the long waves twice a day. We have been doing so for quite a number of years.

Prof. Mesny. — Captain Bureau says that it would be desirable to discuss next year the best method to arrive at a number for definition of static activity. He calls it activity because it concerns the intensity and also the number. He has described at a previous meeting, for example, a method by which one integrates atmospherics with a condenser. A good question to be discussed at next year's meeting will be, the best method of making measurements of atmospherics.

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PAPERS
OF THE
General Assembly held in Washington
in October 1927

FASCICULE 6

TRAVAUX DE LA COMMISSION DE LIAISON
AVEC LES OPÉRATEURS, PRATICIENS ET AMATEURS
(TEXTES ORIGINAUX)

PAPERS OF THE COMMISSION
OF COOPERATION WITH THE AMATEURS
(ORIGINAL TEXTS)

PUBLIÉ PAR LE SECRÉTARIAT GÉNÉRAL DE L'U. R. S. I.
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International Union of Scientific Radio Telegraphy

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Recueil des Travaux de l'Assemblée Générale tenue à Washington en Octobre 1927

Papers of the General Assembly held in Washington in October 1927

TRAVAUX DE LA COMMISSION DE LIAISON AVEC LES OPÉRATEURS, PRATICIENS ET AMATEURS

(Textes originaux)

La Commission de Liaison avec les Opérateurs, Praticiens et Amateurs a été instituée par décision de l'Assemblée générale de l'U. R. S. I. tenue à Bruxelles en 1922.

Les travaux de cette Commission, effectués par des spécialistes appartenant à chacun des Comités Nationaux de l'U. R. S. I., ont fait l'objet de rapports à l'Assemblée générale de l'U. R. S. I. tenue à Washington, en octobre 1927. On trouvera, dans ce Fascicule 6, le texte de ces rapports et discussions.

La Commission de Liaison est composée des savants suivants :

PAPERS OF THE COMMISSION OF COOPERATION WITH THE AMATEURS

(Original Texts)

The Commission of Cooperation with the Amateurs has been instituted by the decision of the General Assembly of the U. R. S. I. held in Brussels in July 1922.

The works of that Commission, made by specialists of every National Committee of the U. R. S. I., have been discussed and summarized at the General Assembly of the U. R. S. I. held in Washington, in October 1927. This Part 6 includes the complete discussions and reports.

The Commission of Cooperation is composed of the following savants :

Belgique :	M. R. B. GOLDSCHMIDT, Professeur à l'Université de Bruxelles.
	Le R. P. LUCAS, Professeur au Collège de la Paix à Namur.
France :	Commandant JULLIEN, du Poste de T. S. F. de la Tour Eiffel.
	M. P. BRENOT, Directeur de la Société Française Radioélectrique.
Great Britain :	Mr. CAMPBELL SWINTON.
Italie :	M. le Professeur VANNI, Directeur de l'Institut Militaire de T. S. F.
	Colonel BARDELONI.
Norvège :	Capitaine B. L. GATTWALDT, Directeur Technique de « Norsk Marconi-Kompani ».
	Capitaine F. BUGGE, Chef du Laboratoire Radiotechnique de la Marine.
Portugal :	Lieutenant Fortunato PIRES DA ROCHA, Chef du Laboratoire Radiotélégraphique de la Marine de Guerre.
United States :	Prof. A. E. KENNELLY, of the Harvard University.

Les séances de la Commission, à Washington, étaient présidées par M. le Prof^{r} Vanni.

The meetings of the Commission, in Washington, were presided at by Prof. Vanni.

Meeting of October 17, 1927

Prof. VANNI, Chairman.

Prof. Vanni. — Ainsi qu'il est bien connu, dans la réunion de notre Union Radiotélégraphique Scientifique Internationale faite à Bruxelles en 1922, et dans le but de coordonner les activités des expérimentateurs et des techniciens de la radiotélégraphie, on créa quatre Commissions internationales dont les trois premières avaient à étudier les problèmes se rapportant aux mesures, à la propagation des ondes électromagnétiques, à l'étude des statitisques, et la quatrième (dont on voulut bien me confier l'honneur de la présidence) avait pour but de coordonner l'activité des radio amateurs et practiciens de manière à en faciliter l'œuvre.

Comme on voit aisément, d'après le titre même de ces différentes commissions, tandis que les trois premières avaient, chacune un but bien défini et déterminé, la tâche de la quatrième Commission, n'était pas aussi bien définie; cette tâche était pourtant la plus difficile à établir et à mettre en pratique.

Pour ce qui concerne notre pays, je dois faire remarquer que, après avoir créé, en février 1923, et d'après l'initiative de l'Académie Nationale des Lincei, le Comité National Italien, sous la Présidence honoraire du Sen. Marconi et la Présidence effective du Sen. Corbino, Directeur de l'Institut physique de Rome, on songea immédiatement à faire de ce Comité, un organe technique intermédiaire entre les opérateurs, amateurs, et les autorités officielles de la radiotélégraphie; dans ce but, le même Comité prit en examen les nombreuses demandes faites par les radio amateurs pour l'installation des postes de réception et de transmission, soit à but d'études, soit à l'occasion des concours pour les essais transocéaniques à ondes courtes qui eurent lieu dans les années 1923 et 1924. En même temps, notre Comité étudia et proposa quelques directives techniques propres à régler les installations radioélectriques des Instituts Universitaires et privés.

L'efficacité de l'œuvre du Comité National Italien, en ce qui concerne la coordination des travaux des practiciens et des amateurs, ne tarda pas à se manifester : à Rome, à Milan, à Turin, à Naples, on procéda à la formation d'un certain nombre de sociétés d'amateurs et on encouragea l'œuvre, soit en établissant des concours avec prix, soit en donnant, aux personnes qui en faisaient demande et étaient dans les conditions voulues, les bâtiments, les appareils et même les moyens d'expérimentation et de recherche. C'est justement à la suite de l'œuvre efficace et enthousiaste des opérateurs Italiens qu'il fut possible de réaliser, en travaillant sur ondes courtes et avec des dispositifs très simples, la liaison bilatéral entre l'Institut Central Radiotélégraphique de Rome, et le poste de Gisborne en Australie aux antipodes, et aussi, celle entre l'Usine Militaire Radiotélégraphique et un poste de la République de l'Équateur.

L'activité des Associations d'amateurs ainsi créés, se manifesta encore dans la technique des transmissions radiophoniques sur ondes courtes, et ce fut ainsi qu'eurent lieu les essais faits par la société ADURI de Milan (Association des Radio Amateurs Italiens), pour l'utilisation radiophonique de l'onde courte de 45 mètres. Ces essais qui sont toujours en cours d'étude, sont publiés dans le *Bulletin radiotélégraphique de l'Armée Italienne* du mois de juillet 1926.

Il est inutile de dire que, par des expériences suivies de radiotélégraphie et radiotéléphonie faites à des époques fixées d'avance, nous avons sollicité la collaboration des amateurs d'autres pays, soit en Europe, surtout en France et en Angleterre, soit au-delà de l'Atlantique avec les Sociétés d'amateurs des États-Unis. Nous avons eu partout un très favorable accueil à l'égard de cette collaboration; mais qu'il me soit permis de rappeler les échanges de vues très efficaces faits en particulier avec l'American Radio Relay League et avec la International Amateur Radio Union. C'est pour moi une très grande satisfaction de rendre ici hommage au Président de ces Sociétés, M. Hiram Percy Maxim, au Vice-Président, M. Gerald Marcuse, et au Secrétaire, M. Warner, à l'amabilité et à la cordialité desquels nous devons la possibilité d'une entente commune et d'un travail de collaboration auquel il sera sans doute réservé une grande importance dans l'avenir.

Ainsi que j'ai eu l'honneur de le dire au commencement de mon rapport, le travail de la IV^{me} Commission était le plus difficile à mettre sous forme concrète parce qu'il était le plus indéterminé. Nous avons commencé à faire quelque chose; mais il ne faut pas se dissimuler qu'il reste encore beaucoup à faire. A côté du travail croissant et se perfectionnant sans cesse des trois autres Commissions, il faut développer et perfectionner aussi celui de la nôtre. C'est un travail qui a, peut-être, le plus de tous, un caractère international et c'est pour cela qu'il est le plus difficile à établir. Je serais bien content d'écouter vos idées à cet égard; un certain nombre de propositions ont été faites ou vont être faites, en particulier, par la « International Amateur Radio Union ». Pour ma part et pour ouvrir la discussion je commence par vous soumettre cette proposition qui a déjà donné de bons résultats dans notre pays. Puisque malgré le progrès merveilleux de la science et de la technique des radiocommunications, il existe encore beaucoup de questions à étudier, beaucoup de problèmes à résoudre, je propose qu'un certain nombre de questions soient choisies par une sous-commission qu'on pourra nommer et qui en fera un choix, pour pouvoir les proposer comme des thèmes d'un concours international, avec prix, ouvert aux radio amateurs et opérateurs de tout le monde. Si vous acceptez cette manière de voir, on pourra étudier les différentes questions du concours et ensuite les modalités pour mettre les concurrents des différents pays, en condition d'y participer.

Général Ferrié. — Professor Vanni has made a very interesting report and told us how they operate in Italy. It would also be interesting to ask what they have done in other countries, perhaps in the United States where the work has been very interesting, in England also, and in France. It would be well for us to find out what has been done in the nations here represented and in other countries if anyone has that information.

Prof. Vanni. — I agree with General Ferrié's suggestion, and will ask for statements from those who are conversant with the radio work done between amateurs and scientific people in their countries.

Dr. Dellinger. — To discuss this question properly, it is important that we understand what we mean by amateurs. The term is used in different senses in different countries and also in the same country, so I will state what I mean when I speak of any particular group of persons. The American Section of the Union has not itself undertaken any program with the cooperation of the amateurs but has on one or two occasions been a means of bringing to the attention of experimenters certain programs which had been initiated by the Bureau of Standards.

Turning to what has been done along this line under the auspices of the Bureau of Standards, first of all, about four years ago a rather extensive program of cooperative observations on fading was undertaken with the aid of the transmitting amateurs, that is, persons who are the members of the American Radio Relay League and who devote themselves to transmission of messages. These are what are most commonly called « amateurs » in America. About 200 of these persons, mostly young men, assisted by taking observations of apparent loudness of signals and known variations on an arbitrary scale.

This was followed two years later by a somewhat similar investigation, the first investigation being devoted to determining some of the elementary characteristics of fading and the second to a more elaborate investigation. Both of these

investigations are perhaps known to some of you as both have been published.

It appeared to us at the Bureau of Standards that it would not be possible to go much farther with cooperative investigations with the assistance of this particular type of amateur because their customary method of working does not involve the use of instruments, and that to obtain data worth while in the future it would be necessary to work with groups of observers how actually make measurements with instruments. Therefore a little over two years ago the Bureau determined to enlist the support of experimenters as far as possible on a further investigation of fading, but this time working in a quantitative manner with instruments. It happened that just at this time apparatus of a simple type had become available through the work of Mr. Pickard and Mr. Shaw of Boston, and furthermore this was just before the eclipse of 1925 and Mr. Pickard had begun the work with a group of experimenters to observe the eclipse. The Bureau cooperated with Mr. Pickard and subsequently organized experimenters to the number of about 40 scattered around the eastern half of the United States to take measurements of fading during the sunset period and certain other times. These experimenters were university workers and in some cases workers in industrial laboratories. The results of the investigation have just been published (Bureau of Standards Scientific Paper, n° 561).

The upshot of the experience of the Bureau of Standards along this line has been that the present status of cooperative radio investigation requires observations with measuring instruments, and that only a few persons can be enlisted in such work. It is necessary to give them the most precise instructions, as we cannot rely on their having much imagination or much time to work out methods. Insofar as qualitative investigations go where you can depend on estimates of audibility, the Bureau of Standards has found that the transmitting amateurs can and will give very faithful dan reliable service. Whether they would continue to after many months or years is somewhat doubtful because you must always have a program which maintains the interest of those who participate. As to whether the transmitting amateurs would be available for future transmissions I do not know, but there are some representatives of the American Radio Relay League here today, and they might give us some observations on that point.

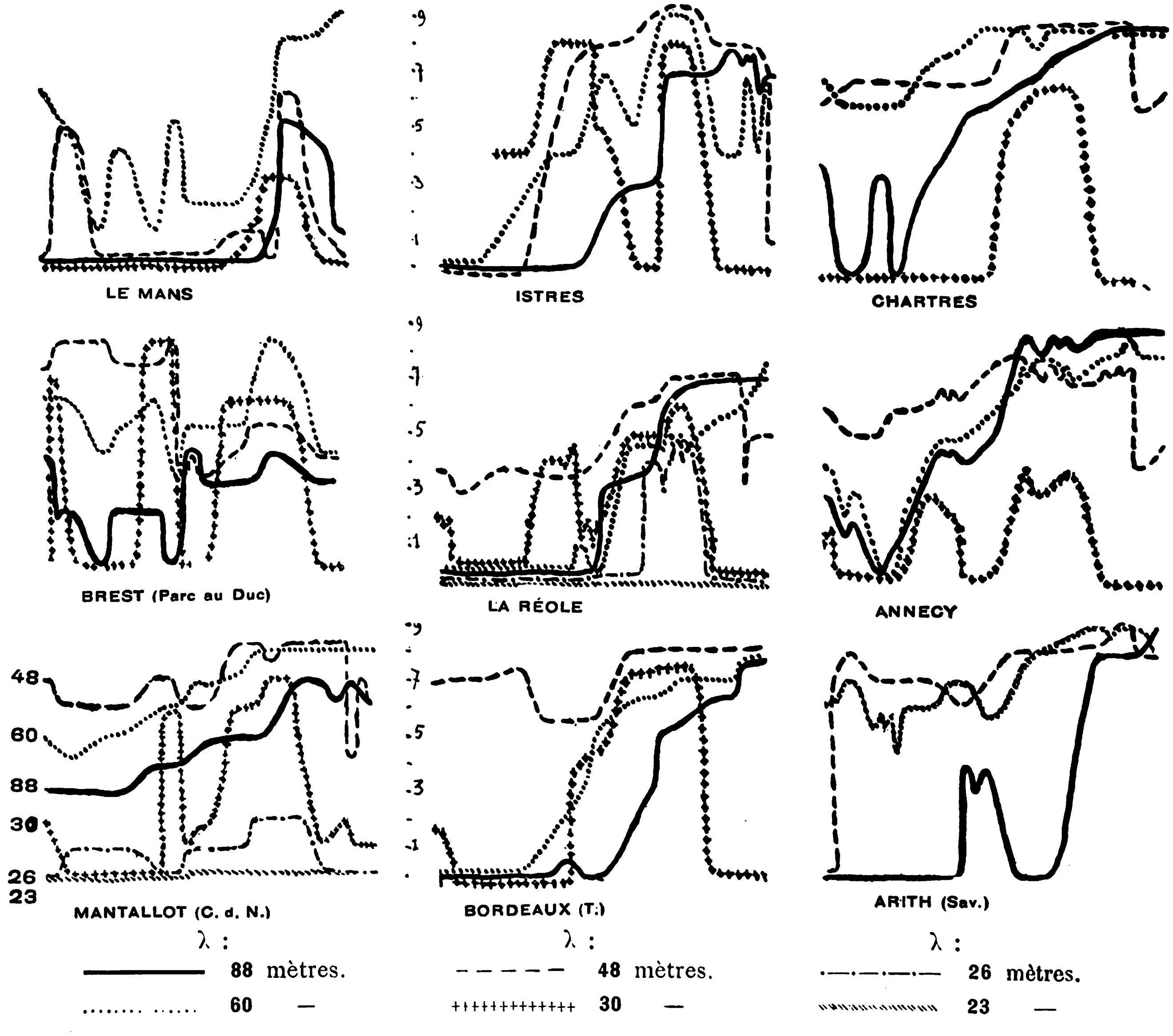
Prof. Appleton — I was very much interested in Dr. Dellinger's remarks because our experience in England is almost identical. The Radio Research Board some years ago attempted to enlist the service of amateur observers in a cooperative study on fading, but unfortunately the first series of results was very nearly worthless but it was sufficient to enable the Radio Research Board to find ànd pick out some of what we call « selected amateurs » who were capable experimenters and the Radio Research Board furnished these amateurs with a table galvanometer so that they could get relative measurements of the field strength, and that appears to me to be one of the things on which our amateurs could cooperate. I for one should feel diffidend about asking them to measure absolute measurement of the field strength. I would like to point out the kind of circuit used by our amateurs. It was simply a radio frequency amplifier with a detecting stage and the detecting current passing through a small galvanometer which costs in England about 3 or 4 pounds. The results have been very oncouraging because in the recent total solar eclipse on June 29 this series of amateurs took most accurate observations of fading during the period of the solar eclipse, again merely relative readings. They were arranged in a line from the north to the south of the country so we were able to get readings at different distances. It seems to me that results of that type are of great scientific value. That again fits in with Dr. Dellinger's conclusions that we must either ask these amateurs to buy their own galvanometers or provide them wit a galvanometer. Then again, it seems to me, we must have objective readings and not subjective. That work, of course, was on the broadcasting wave lengths where one half of the experiment is always working. It is merely the receiving end the must consider. The short wave work in Great Britain has not met with much success until the solar eclipse and there was a proper cooperation organized by the Radio Research Board, but the results have just been published within the last few weeks so I can not give you the results themselves.

Dr. Dellinger. — Apropos of one remark of Dr. Appleton's on selecting observers, that is always necessary. A program is never successful if simply thrown open to any observer. Perhaps because warned by early British experiments, the Bureau of Standards was careful to obtain only persons as observers whose record had been examined and whose indications were carefully scrutinized and observers in all cases were selected men. I should supplement what I said before by saying that the Bureau of Standards has not been the only agency in this country that has organized projects of this kind. There have been some others wich have had some success and many people have thought of the idea of utilizing the broadcast listeners. We have three different kinds of possible observers : one is the transmitting amateurs of which there are about 18,000 to 20,000 in the United States; broadcast listeners, which number millions; and another is the very small group of persons experienced in radio measurements. There have been a few attempts to make use of the broadcast listeners during the eclipse of 1925 when one of the magazines broadcast a request to listeners to take observations. The net result briefly is that observations by the broadcast listeners are of practically no value.

In the field of short waves, inyestigations have been conducted quite extensively by the General Electric Company and the Navy Department with the assistance of transmitting amateurs and persons who were accustomed to carry on radio traffic. In these experiments, the observers were not only Americans but persons located all over the world, and briefly speaking, those projects were quite successful, particularly the one organized by the Navy Department. The work published by Dr. Taylor has been largely obtained on work reported by amateurs and located all over the world. Something along this line was done by the General Electric Company, and information of value was also obtained.

Captain Bureau. — I speake as Chief of the Communication Service in the French Meteorological Service and in charge of the liaison of the amateurs. I am extremely interested to hear the remarks of Dr. Dellinger and Dr. Appleton and they coincide with my own experience in that they found it necessary to select the amateurs. We were lucky enough to enlist the cooperation of a few extremely devoted amateurs and very good experimenters. Using stations of the Navy and Meteorological Service, a certain number of experiments have been made. I am entirely in accord with the remarks made as to the interest of quantitative mesurements, but wish to say that I have been able to get very interesting results from purely qualitative measurements. The first series of experiments dealt with the diurnal variation of the transmission of propagation of signals and these experiments were made on several wave lengths from 80 meters down to something like 23. I have here a chart (fig. 1) showing the results of these experiments dealing with the diurnal variation. The curves relating to the various wave lengths are marked in colored pencils, the variation of the colors corresponding to the color spectrum in the same decreasing order as the wave lengths used. A second series of experiments consisted in trying to plot on a map of the country the location of the stations wich heard a certain given signal at a certain hour. It was not at all evident from the first that these various stations who did not hear a signal would come together in a given region, and the actual fact was that they did; which is a very interesting result and has been obtained entirely with qualitative measurements. Of course if we could have quantitative measurements it would increase the value of these studies as we should then be able to draw contour lines of equal intensity instead of bieng limited to indicating whetter the signal was or was not heard. In the third series of experiments it was suggested that the amateurs should listen to the short wave emission of certain mobile stations from ships sailing from France or from airplanes, some of which were above a given region and others of which were leaving France and making a tour around the country or into North Africa. This kind of experiment is especially interesting to the âmateurs because the amateur can see the variation in what he masures and sees the ship going in or going out as the ship reaches certain regions, which is not the case where the analysis has to be made in some office. The fourth series the Meteorological Service started was called atmospheric soundings; a small transmitting set was used, weighing not more than 5 ponds and equipped with an automatic clockwork to control the key. This was attached to a small balloon and sent up in the air, and the amateurs recorded the intensity of the signals. A few of these experiments have been made in which the balloon reached as high as 13,000 meters in the atmosphere. Other experiments of relative intensity of signals have been made by the amateurs on the occasion of the solar eclipse, and this should show how desirable an international comparison on these subjects would be since it would certainly have been profitable in the British and Frensch amateurs had worked together on this eclipse.

FIG. I. — **Variations des intensités de réception de six ondes émises à Paris le 19 mai 1927 de 11 h. 40 à 23 h. 45.**



Dr. Goldschmidt. — Speaking not as Secretary General of the Union but on behalf of the Belgian group of the U. R. S. I., I wish to say a few words on this jubject. I have found quite a few difficulties owing to the fact that Belgium is not a big country, and owing particularly to the fact, which may possibly have been duplicated in other countries, that I find a certain difficulty in having college professors and high school teachers who have been willing to cooperate and who would have certainly given very interesting results, — in enlisting these high school and college professors in the same group as the radio amateurs. They did not feel that their status as college professors enabled them to work in cooperation with the amateurs. This might possibly come from the fact that there is a rather amateurish sense to the word «amateur» in French. We use this word as a person who is not very well versed on the subject and has a rather vague acquaintance with radio. I have wondered if some other name might not help the situation in Belgium and possibly in other countries, and would like to hear if you have had similar experiences.

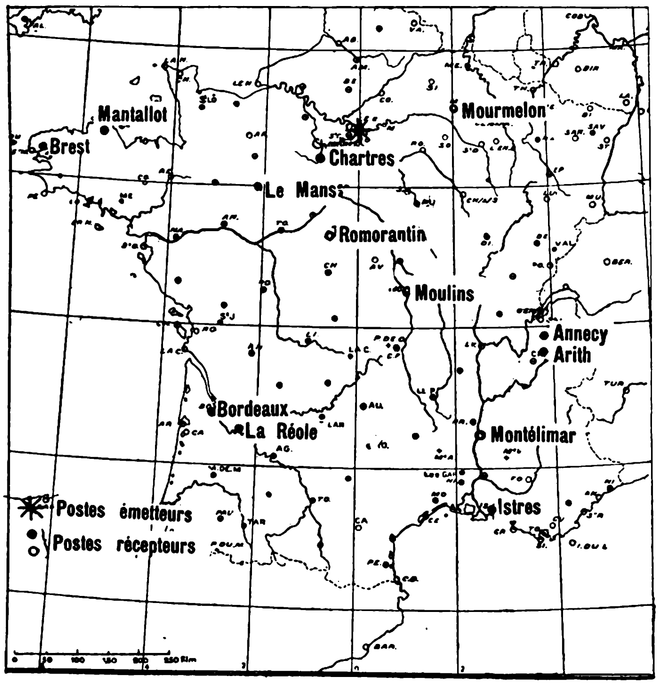


FIG. 1*a*

I was highly interested in the remarks of Mr. Appleton and am very glad to hear that some simple apparatus has been designed which would enable us to get relative quantitative results. I also feel that the absolute measurement of the field might perhaps be beyond the present possibilities but would like very much to have objective results obtained. For instance, my experience in Belgium has been that the results which have been handed in have been useless. Certain results on the same speech and at the same time give an entirely different result by the different observers, which very likely comes from the variety of apparatus used. I would like to have one simple kind of apparatus standardized and, if possible, not only in my own country but the world over, in order to be able to compare the results obtained, and I think that perhaps a subcommittee could come to an agreement on such an apparatus. I also think that it would be very desirable to have not only the apparatus standardized but also the vacuum tubes. I have been fortunate enough in Belgium to interest the Phillips Company whom I have always found extremely willing to lend a hand to scientific research, and this has enabled me to get an excellent group of homogeneous lamps which has been found very useful.

Prof. Vanni. — I think the remarks of Dr. Goldschmidt are extremely interesting and valuable and I think we should do something about it. Before leaving the subject I want to know if there is any other member who has information on this subject about his own country.

Dr. Van der Pol. — It has been remarked that it is not very advisable to get only approximate qualitative results from amateurs but that it is more advisable to have quantitative measurements. When we give the amateurs galvanometers and we give them to people who can work with them, it is very difficult under these circumstances to define who is an amateur and who is not. If you define an amateur as somebody who is interested in wireless, I think all of us present here are amateurs. On the other hand, when we give what we usually call amateurs, a galvanometer, thy become scientists, and it is my impression that the boundary line between amateurs and scientists is very much washed out in this way, and in order to get further discussion it would perhaps be advisable to have some definition of the word.

Dr. Appleton. — We might call a man an experimenter when he has a galvanometer.

Dr. Goldschmidt. — The word amateur is very often in controversy and the real distinction that you want to make is between the reliable and non-reliable amateurs. The radio clubs should put them in these two classes, and I would like to see something done by radio clubs to members when they make scientific experiments. These experimenters could be put in line with the high school teachers and college professors that we have and make very useful tests.

Prof. Vanni. — I agree with the remarks of Dr. Goldschmidt. The same difficulty is met in Italy. Without going deeply into the subject, I think that among these observers, as you call them, we shall have to make at least two categories, those who can be relied upon to make good qualitative observations and those who can make quantitative measurements with an acurate scientific instrument.

If we are all in accord, as we seem to be, on the general ideas presented this afternoon, then the time would be ready to get to work on a definite program of experiments, deciding what questions ought to be put before this body of observers and what kind of apparatus should be used and what methods of experiment should be recommended. We might adjourn, and begin at our next meeting the consideration of these questions.

Dr. Dellinger. — Before adjourning, I would like to ask that you call on Mr. K. B. Warner, representing the American amateurs, whether he cares to say anything on this subject as it is possible he might not be at our next meeting.

Mr. Warner. — I have only a general idea of the nature of the problems and so I am able to comment on them only in a general sort of way. I have had some ten years' experience in the administration of the affairs of the ordinary variety of amateur, what might be called the «amateurish amateur», and perhaps I can tell you something about him. I am glad to see that you have no illusions on the question which would lead yo u to too great expectations. The experiences and statements in general of Dr. Dellinger and Captain Bureau are quite acurate. There is not a very large number of American amateurs qualified to make qualitative measurements. We have so many in this country that the percentage is going to be found disappointingly small. I am certain it is quite true that your observers will have to be selected. However, among so many, some 16,000 or so in this country, and surely 25,000 in the world, there must be many hundreds who are generally able to learn enough to be of assistance in this work able and willing. The amateur wants to help. All around the world he is interested in demonstrating that he is capable of being of assistance in the advance of the art. Most of the amateurs of the type that I know best are primarily concerned about two-way communication and the development of apparatus for that purpose, experimental work and development work, perhaps, but largely in connection with the maintaining of a station for two-way communication and that development work with them is a hobby. The amateur of this type is a person who does something else in the daytime and follows this only to the extent that it interests him. It is therefore necessary when you desire his cooperation as an observer o make the work of a nature most likely tot appeal to him. I feel that I canal most say upon looking at a proposition whether or not it would succeed in getting the cooperation of the American amateur. If the subject interest him there is no length to which he will not go to assist; if of no interest to him it will be very difficult to get any assistance. I doubt, for example, if it would be possible to find many American amateurs willing to cooperate in a program which involved determining the apparent direction of static. On a project which involved two-way communication or the employment of his own apparatus, enthusiastic and valuable cooperation could be got.

I also have in mind that Colonel Gold of the international bureau for synoptic weather information is in communication with Senor Moya, President of the Spanish Section of the International Amateur Radio Union, requesting a program to procure Spanish amateur cooperation where they would employ their own transmitters in transmitting local data.

I, of course, do not know what the specific problems are in which you are interested, but I wish to offer cooperation of the amateur body in this country and to offer to be of all the assistance I possibly can be with respect to securing cooperation in other countries, inasmuch as I am also the Secretary of the International Amateur Radio Union.

Prof. Vanni. — I thank Mr. Warner very much for the remarks which he has made. We think his experience would be most valuable to us in our next conference to determine whether or not a given program could awaken the interest of the American amateur.

Mr. Warner. — I should be very happy to be of any possible assistance.

Mr. Madsen — I think we might refer to a small subcommittee consisting of those members who have had experience in this work the question of drawing up a program so we might have something definite to discuss at our next meeting.

Prof. Vanni. — Will Mr. Warner accept membership on such a committee consisting of five or six people?

Mr. Warner. — I could only be of assistance, it seems to me, in saying whether there is a possibility to interest amateur assistance in this particular problem and not in that, and what methods of attach would be best to get it.

Prof. Vanni. - We think your assistance would be extremely valuable and would you accept to be on the subcommittee?

Mr. Warner. — I would be glad to.

Messrs. Mesny, Appleton, Dellinger, Bureau, Pickard, and Warner were appointed a subcommittee to meet Wednesday morning, at 10 o' clock, to discuss this matter.

Meeting adjourned at 4 : 30 P. M.

Meeting of October 20, 1927

Prof. VANNI, Chairman.

Prof. Vanni. — We had an interesting meeting of our Subcommittee yesterday. Captain Bureau suggested a plan for organization of a project by this Commission in connection with amateurs. If the Commission wishes, the plan that Captain Bureau has submitted will now be read.

Proposed Plan of International Experiments on Propagation of Short Waves.

Presented to Commission 4 of the U. R. S. I.

The small committee nominated by Commission 4 (composed of Messrs. Vanni, Appleton, Dellinger, Pickard, Goldschmidt, Le Corbeiller and Bureau) met on October 19 at 10 A. M. They recommend the following plan to Commission 4.

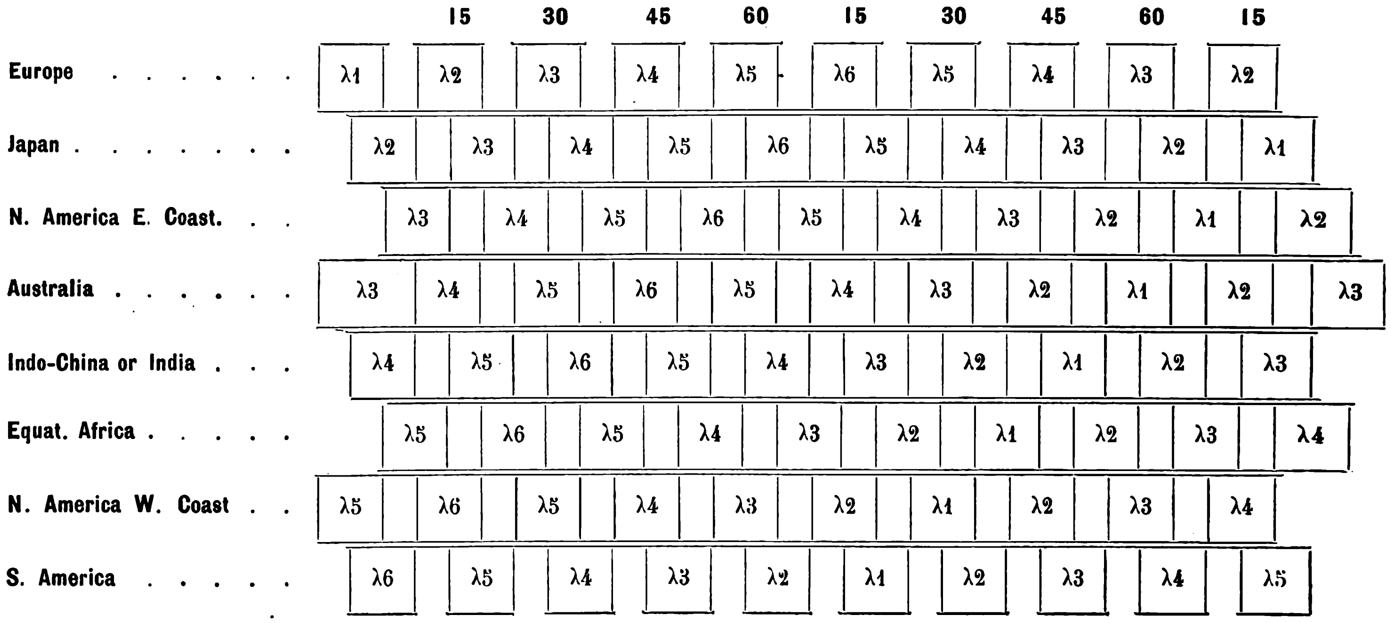
The cooperation of the amateurs will be especially useful in researches of a statistical character, i. e., where it is desired to obtain an average of the totality of the phenomena. The number of variable parameters in the propagation of short waves is very large, the principal ones being the latitude and longitude of both the receiver and transmitter, their geographical location, their altitude, the time of transmission, and the wave length. A geometrical presentation of the propagation phenomena would therefore only be feasible with the aid of families of surfaces. During the tests one would be obliged to study the consequences of the variation of these parameters, leaving the others as constant as possible.

It will be especially interesting to study the influence of wave length if one wishes to attract and sustain the interest of the amateurs. Indeed in the study of the propagation of one wave only there will be regions where that wave shall not arrive and where the amateurs will only be able to report negative results. This would be discouraging for the amateurs. On the other and, a certain number will not care to report those negative results because they may be afraid that they be due to a faulty reception. On the contrary if several wave lengths on a sufficiently broad band are studied at the same time there are very great changes for some of these waves being received by each amateur during a part of the test. This amateur will then be able to see himself one of the effects of propagation (the influence of frequency) and he will be very much more likely to report the cases when he has not received anything.

The tests which we suggest can be classified in three groups :

First Group. — Statistical study of the diurnal variation of several waves simultaneously. These tests should extend for 24 consecutive hours with at most short interruption of about one or two hours. They will therefore demand considerable strain from the amateur and should only be made several times per year. It is desirable that these should coincide with the days put aside for international soundings of the high atmosphere.

These transmissions will be made in 8 different places which will be : 1) Europe; 2) Japan; 3) East Coast of North America; 4) Australia; 5) Indo China; 6) Equatorial Africa; 7) West Coast of North America; 8) South America.


15 30 45 60 15 30 45 60 15
Europe λ1 λ2 λ3 λ4 λ5 λ6 λ5 λ4 λ3 λ2
Japan λ2 λ3 λ4 λ5 λ6 λ5 λ4 λ3 λ2 λ1
N. America E. Coast. . . λ3 λ4 λ5 λ6 λ5 λ4 λ3 λ2 λ1 λ2
Australia λ3 λ4 λ5 λ6 λ5 λ4 λ3 λ2 λ1 λ2 λ3
Indo-China or India . . . λ4 λ5 λ6 λ5 λ4 λ3 λ2 λ1 λ2 λ3
Equat. Africa λ5 λ6 λ5 λ4 λ3 λ2 λ1 λ2 λ3 λ4
N. America W. Coast . . λ5 λ6 λ5 λ4 λ3 λ2 λ1 λ2 λ3 λ4
S. America λ6 λ5 λ4 λ3 λ2 λ1 λ2 λ3 λ4 λ5

Au total 2 heures 50 minutes de transmissions.

The principal meteorological services in the world make every year a certain number of soundings using sounding balloons which carry recorders of temperature, pressure, fumidity, etc. In order to get better efficiency for those costly experiences those soundings are made by all countries at certain days of the year or during certain periods. One has therefore for these particular times an exceptionally complete documentation on the meteorological condition of the atmosphere between the ground and the stratosphere and in certain cases of the lower layers of the stratosphere.

In each group transmission should be made on six different wave lengths chosen approximately in the following bands :

Band *a*) between 14 and 16 meters.
Band *b*) between 18 and 20 meters.
Band *c*) between 26 and 31 meters.
Band *d*) between 38 and 45 meters.
Band *e*) between 50 and 60 meters.
Band *f*) between 70 and 90 meters.

Each of these emissions would last ten minutes, the emissions within each group being separated by an interval of five minutes, but the emissions of two different groups overlapping five minutes so as to give the following scheme : (Shows schematic diagram.) In this schematic diagram the interval between two vertical lines is 5 minutes. The total of the 48 emissions would last 2 hours, 50 seconds. Such transmission might begin at 0 hour, 3 A. M., 6 A. M., 9 A. M., 12 noon, 3 P. M., 6 P. M., 9 P. M., which gives 1 hour and 10 minutes rest between each group.

Each person following these tests might easily follow four of them, either by following four groups of countries, or following a certain band of wave lengths, or following certain bands on certain transmitters and other bands on other transmitters. It would be desirable that the audibility of reception be distributed among several receiving sets as little apart as possible in order to have practically the same density of listening stations on the same emissions. This ought to be easily organized in each country. In order to facilitate the dissemination of the transmissions they would be composed of one letter or one numeral infinitely repeated and always the same for each group whatever its frequency, but different from one group to another. During the transmission a group of five numerals (or five letters) will be incorporated in the transmission of the letter (or numeral) characterizing the transmission so as to insure control.

Second Group. — Synoptical and geographical study on certain hours on different simultaneous frequencies.

Here the test would simply coonsist in plotting on maps the zones of the surface of the earth where a given transmission is heard or not at a given hour. When we shall dispose of standard sets comparable to one another in a big system of receiving stations it will be in addition feasible to trace contour lines joining the spots where the reception is the same.

In order to eliminate or at least bring to view the existence of meteorological influences it would be useful that such emissions be made three days in succession. Moreover it would be desirable to have them inserted between the hours of international meteorological observation which are also those of the meteorological maps. The total groups of transmissions would be like the preceding ones. They would take place about six times a year on three consecutive days, the groups beginning at 0 hour, 6 hour, noon and 6 hour Greenwich time.

Third Group. — The transmitting station would be mobile either on the surface of the water (in case of ships) or in altitude (airships) or sounding balloons. This is also conducive to attracting and sustaining the interest of the amateurs.

No definite program suggested here but it is recommended that agreement should be made in writing so that in a test which interests now only on a national scope might have an international one.

Prof. Vanni. — You have all heard the report made by Captain Bureau. I think it is very interesting but before I make any observations on it myself, I would like to hear the remarks some of my colleagues here would like to make on this report.

Mr. Warner. — Wat kind of record would be desired from an amateur observer? Just to say if he has heard the transmission or not?

Prof. Vanni. — Yes. That is the principal idea of the plan proposed by Captain Bureau. The amateur would simply have to report whether or not he has received a given transmission. I think that the idea Captain Bureau had in mind is the present consequence of making transmissions on a given wave length, which is that the amateur does not hear it and he is both discouraged and rather ashamed to report he has not received it, but if you send several wave lengths at the same time he will undoubtedly hear some of them and will be interessed and encouraged to report the whole thing.

Mr. Warner. — I think it is very interesting. I was thinking, as the matter was developed, that there are many amateurs who have already cooperated in such a plan to determine the performance and skip distance effects of various wave lengths but then I realized that that had never been done except on a very small scale and here is an opportunity for the amateurs of each nation to do something on an international scale where the results have never before been determined, and I think that the main proposition as explained under group 1 and also similarly under group 3 is extremely interesting and have no doubt it would be possible to secure enthusiastic amateur cooperation from every country. However, I question whether so complete a cooperation would be secured in a three-day test as proposed in group 2.

Dr. Goldschmidt. — I wonder if it would not be desirable to have the amateur indicate what kind of a receiver he is using for these tests. I realize that this is a qualitative test and that therefore we do not have to go into the exact specification of a standard receiving set as would be necessary in the case of quantitative experiments. I wonder whether in the present state of receivers quite different results may not be obtained by equally good amateurs at the same spot with different apparatus using different amplifications, and I wonder if something to cover this should not be inserted here and what.

Mr. Warner. — I should think it would be very important to know whether radio frequency amplification has been used or not and some brief description of the nature of it. We expect in the near future to have quite successful radio frecuency amplification at short waves. I also think it woulb be quite important to know the nature of the receiving antenna, as to whether it was a vertical aerial or a horizontal aerial. Dr. Pickard has shown that there aer great differences in the reception of the two under these circumstances.

Mr. Pickard. — I think it is very advisable that some description of the complete receiver, not in any great detail, but the type of aerial and type of receiver, be given.

Dr. Goldschmidt. — I wonder if it might not be a good plan to recommend to those amateurs who would be willing to follow the recommendation, a certain type of set or aerial. This could be done at the same time that we ask them what circuit and antenna they are actually using. I think that if we could agree to recommend some kind of circuit there would be a likelihood of securing more comparable results.

Mr. Warner. — I would not hope for great success in that direction. It would undoubtedly be of interest to them to know what equipment the U. R. S. I. would suggest as best suited for this kind of work, inasmuch as all amateurs desire to have that equipment which best talent thinks is superior. However, I think you would have to trust the amateur to report on his own equipement, because the chief incentive to the amateur is that he will be highly interested to test his skill and his equipment on the many transmissions from all parts of the world on the different wave lengths. He would like to know what he can do with his own apparatus, and particularly since his associates are competing with him.

Dr. Goldschmidt. — It should be well understood that this is not a competition to see who can receive the farthest. The results must be comparable to make the test of value.

M. Warner. — I think the competitive idea is necessary. An amateur would be interested in giving you quite as accurate a statement as he was capable of, but nevertheless he would hope that he could excel the other people that are joining with him in these tests. He would hope that when the results were published, it would be said that he was the one who heard the signals the most times.

Dr. Goldschmidt. — I am impressed by the variety of results which you woulb be able to get with various sets, and I would suggest some kind of limitation.

Général Ferrié. — I do not think we should go into that too much. We might follow the su5gestion of Mr. Pickard and smiply ask the amateurs to report the kind of receivers they use and the kind of aerials, and see what results we get on the ist or second tests.

Dr. Goldschmidt. — It might be well to publih the results.

Mr. Warner. — It would be interesting to see if the particular amateur how reported that he had one or two stages of radio frequency, was getting better results. It would be interesting also to see if the occasional amateur who had a superheterodyne was getting better results. Also you might find a group where many amateurs were reporting with the same type of apparatus with vertical antennas, but with one employing a horizontal antenna and getting results of a different order.

I would like to inquire whether it would be possible to code these transmissions, as otherwise your only report would be the statement of the amateur. I am afraid some amateur might become too enthusiastic and report something he had not heard. I understand the suggestion of Captain Bureau to be that each station would send a characteristic letter or numeral for the test, the same for the different frequencies, and included in the text would be a code group of five letters (or numerals), different for each wave length and changed from test to test.

Prof. Vanni. — In my country the several amateurs like to work independently in the hope of discovering something new and it is interesting if we can plan a test that would enlist their cooperation in some kind of group work.

About the bands of frequencies suggested by Captain Bureau, the first of these bands lies rather low and the second is between 18 and 20 meters. Then, on certain condensers there is a great difficulty in tuning on such a low wave length with the condensers usually employed. I do not wish to make any definite proposal in regard to this, but I wish to point out the difficulty and to say that the question has been studied in a report by Colonel Sacco of the Italian Army which I have already pointed out to you. I think that taking into account the remarks made by Mr. Warner and Prof. Goldschmidt on this subject that if results are to be expected on those very short waves a specific type or at least a certain kind of receiving set would have to be recommended.

Dr. Goldschmidt. — There are many different results obtained according to wether you use one type of tube or another; in a test made in Belgium the Philips Radio Compay furnished a group of tubes that were adapted to the particular kind of work that was done and which were closely similar to one another.

Général Ferrié. — I wonder whether such a procedure would be at all possible in this country where you would have so many participants. Mr. Warner says that American factories have developed special types of tubes for short wave work and those tubes will very likely be known by the amateurs who follow the subject. I would like to ask Mr. Warner whether he thinks as a practical thing it would be likely that in such a test the majority of the amateurs would use the same type of tube or not.

Mr Warner. — I do not believe any great number would go into any special equipment. I think, however, you would find the equipment fairly uniform. The average amateur uses nothing more complicated than the autodyne, and is fully equipped to receive wave lengths as low as 18 meters. There are not so many that go down to 14 or 16 meters because that is not a commonly-used amateur band, and it may be that there would be quite a few less that readily receive in that band than in the waves from 18 meters up. However, there is no question about making an autodyne perform with the standard tubes of the world. The average amateur of this country does copy all of these countries as a matter of course.

Dr. Goldschmidt. — It seems to come out from this conversation that the thing to do is to allow any amateur use the equipment he likes, asking him to report what kind of equipment and antenna he uses, and possibly from the results of such a test interesting observations may be got.

Mr. Warner. — I think in that respect that, after all, the thing which determines whether you are going to hear the signal is not the type of apparatus, with the single exception of the difference in aerials, horizontal and vertical, but is the choice of frequency; and the signal is going to be largely dependent on distance and power. The factor which you are interested in is the performance of the wave, and the answer is going to be whether it can be heard or not and that is largely independent of any apparatus. I also have in mind that amateurs will have to fill out blanks.

Mr. Pickard. — I might add a further suggestion; the amateur in this country and also abroad, is today well used to the estimate of signal intensity on the so-called R scale. The scale runs from 0 to 9. It is really a geometrical scale. That means that the difference between the lowest and the highest number of that scale is of the order of several hundred fold, probably enough to make it worth while for an estimate by the amateur of the signal intensity. Instead of a yes or no answer, some estimate of intensity in addition, might be desirable.

Dr. Goldschmidt. — Thus in order to facilitate the analysis of the very large quantity of answers it might be desirable to give a form of answer to be filled in, it being understood that there would be a blank page where the amateur would be welcome to report any other observation that he might have made. Otherwise the analysis of these results would be very difficult.

Mr. Warner. — Will the U. R. S. I. print and distribute these forms?

Dr. Goldschmidt. — Yes, I have money in the Treasury to print these blanks.

Mr. Warner. — I would like to propose that the question of administration is going to be quite a problem, and to ask whether arrangements will be between the National Sections of each country or just who, and to stress the importance of complete instructions that will be understood by the amateurs, and arrangements to get these into the ands of the amateur societies and magazines. It is particularly important to have arrangements for making that information available in plenty of time for publication and notification. There is also involved consideration of ways and means to increase interest in this, as by making the results available so that amateurs might see them.

General Ferrié. — Certainly we cannot expect you to do that hare or now. We might have a permanent subcommittee to deal with that question. It is not our business here to do that.

Mr. Warner. — Your committee migt recommend some of the details which should be incorporated in the plan to insure its success.

Prof. Vanni. — I suggest that the same subcomittee who discussed this plan yesterday might go further into the study of it. Can you be present, Mr. Warner?

Mr. Warner. — I will try to be present.

I do not understand this second group. I do not see how it would work, because, as I understand it, from the same transmitter several wave lengths would be sent simultaneously to get some rather obscure data, and it does not seem to be so valuable as that in the first group. If I am wrong I would like to have it explained.

Général Ferrié. — Captain Bureau will be at the meeting of the subcommittee and will explain it to you then.

Prof. Vanni. — I have received a suggestion from the British members in regard to the relation of atmospherics in the high atmosphere and suggesting a series of tests, one day per month, and as this Commission is charged not only with the work with the amateurs but also with the possible points of contacts between radio and other sciences such as geophysics, and therefore this test might possibly come under the consideration of this committee.

Général Ferrié. — I think it would be better to have the Commission on Atmospherics study this question first and then we might decide what tests we could possibly ask the amateurs to do.

Prof. Vanni. — This suggestion to have this first submitted to the Committee on Atmospherics. The proposal is : Acoustical observation of atmospherics accompanying broadcasting. It is suggested that an international day per month be set apart for observations made by a large body of observers according to the scheme drafted by the Royal Meteorological Committee on Atmospherics held simultaneously on several systems of broadcasting. Continuous recording of statics on a given frequency. It is requested that each country organize a standardized recording of the statics on a small band of wave lengths and on several wave lengths.

Dr. Austin. — The absolute intensity will have to be measured.

Prof. Vanni. — We will pass this on to the Commission on Atmospherics.

Meeting ajourned at 4 : 30 P. M.

Meeting of October 26, 1927

Prof. VANNI, Chairman.

Prof. Vanni. – I shall ask Captain Bureau to give the report of our subcommittee on the proposed work to be done by the amateurs.

Capt. Bureau (reads report). — The subcommittee consists of Prof. Vanni (Chairman), Dr. A. H. Taylor, Dr. L. W. Austin, Dr. J. H. Dellinger, Dr. A. E. Kennelly, Mr. K. B. Warner, Mr. R. B. Goldschmidt, Captain R. Bureau and Dr. P. Le Corbeiller. The subcommittee met on Friday, October 21, to examine the proposition submitted by Captain Bureau.

The first and third groups of proposed tests were approved. It was thought preferable to combine the second group of tests with the first and thus make four tests per year instead of two.

Dr. Goldschmidt suggested that he would have the work of analyzing the reports sent in by the amateurs done in his office and under his direction. This was unanimously accepted by the committee. Is was decided, moreover, that these results would also be sent by the General Secretary of the U. R. S. I. to all the national committees who could analyze them in accordance with their own methods if they so desired.

Dr. Taylor called attention to the existence of simultaneous emissions on high frequencies from Arlington. These were taken into consideration in the organization of these tests.

Mr. Warner offered to address a call to members of the A. R. R. L., by methods at his disposal, and determine at what date the work could begin. It does not seem possible to make the first tests before next spring. However, Mr. Goldschmidt gave information on these tests to Mr. Warner so that he might publish an article in a forthcoming issue of QST attracting the amateurs' attention to the U. R. S. I. in a general manner.

Captain Bureau will work with the Secretary General in the organization of these tests.

Prof. Yokoyama. — I am very doubtful whether we are in a position in Japan to collaborate in the observations because many amateurs are not licensed by the Japanese Government at the present time Also we cannot decide whether we shall be able to make any transmissions in Japan or not. We engineers are quite willing to make such an experiment on an international scale and will try to do anything within our power.

Prof Vanni. — Report is approved. I have here a proposition made by the English Committee which I think comes under Commission IV's work :

Aural Observations on Atmospherics Accompanying Broadcast Matter : It is proposed that on one « International Day » per month, observations should be made by a widespread network of observers, according to a scheme such as is used by the Royal Meteorological Society's Committee, on the atmospherics heard along with matter simultaneously broadcast from a group of broadcasting stations.

Continuous Recording on Fixed Wave Bands: Each constituent country to be asked to install one calibrated continuous recorder operating non-directionally, on a fixed narrow frequency band common to all. The British National Committee can provide a specification of circuits and apparatus for this purpose.

Captain Bureau. — Will not always be the same wave length band in the different countries.

It is very difficult to arrange so that one does not receive at the same time regular transmissions. In France, I have had difficulty.

Dr. Austin. — Why not take a wave length which is long enough so it will be outside!

Dr. Dye. — Of course it greatly restricts the information available.

Dr. Austin. — It would be desirable to find others and that is one thing we are able to do.

Prof. Vanni. — I think it will be found that a wave length which is favorable for one country is not favorable for another country. Do you not think this question should be studied by the Static Committee and then sent to the different National Committees?

Dr. Austin. — It seems to me that is a question that will have to be gone into rather carefully. We can choose one band that is beyond the range of ordinary transmission, there is no question about that, but whether we should ever be able to get real continuous observations on the atmospherics throughout the more generally used wave lengths I am not at all sure.

Dr. Dye. — If one uses a frame aerial would it not be possible to some extent?

Dr. Austin. — No station sends absolutely all the time. You may be able to get out of your registration times when only the atmospherics are in and the station is out. You get a continuous record which would contain both the station signal and the atmospherics. You could get enough time perhaps when the station was not sending so that you would be able to get the strength of the atmospherics.

Prof. Vanni. — We are then in accord on the principal question — that it would be agreeable to take up this question.

Captain Bureau. — I would like to add something. It is very interesting to concentrate on these studies one day each month, but I think it would be equally interesting to make at the same time observations such as are made in France every day in the year. We need not analyze observations in an effort to obtain absolute values of the atmospherics but we will have the diurnal variation of the intensity of the number of atmospherics. Thus we can have two curves, the curve of the variation of the intensity and the curve of the number of atmospherics. And thus we can see at what time there are particularly strong atmospherics and at what time strong atmospherics disappear. It is very interesting to compare these curves from various stations of the same country. In France we have seen that it is always the case that this curve is not the same from all the stations of the country. The coming of strong atmospherics is rarely the same. It is only the same when the weather is stormy. We have worked at wave lengths between 1,200 and 4,000 meters. We have made experiments on the same station on short and long wave lengths. There are times when there are complete differences and there are also differences between diverse short waves. For a difference of about ten meters on the short waves we can have total differences, which proves that the cause is not in the apparatus.

We could perhaps add recording of this nature to the British proposal.

Prof. Vanni. — We shall then send this proposal to the Committee on Atmospherics. Are these observations as proposed by the British Committee, together with the additional proposal of Captain Bureau, confined to amateurs only, or should observations also be made by the meteorological stations of the different countries?

Général Ferrié. — I suggest making an executive committee composed of a few members who will take care of making these arrangements.

Dr. Austin. — I think that might be a good idea.

(Decided that Prof. Vanni, Dr. Austin, Captain Bureau, Dr. Appleton and Dr. Goldschmidt should examine all questions which come before Commission IV.)

Meeting adjourned at 3 : 30 P. M.

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